PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCIII.

PART I.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCIII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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APPENDIX.

THE President and Council of the Royal Society adjudged, for the year 1792, the Medal on Sir Godfrey Copley's Donation, to Sir Benjamin Thompson, now Count of Rumford, for his various Papers on the Properties and Communication of Heat.

PHILOSOPHICAL

TRANSACTIONS.

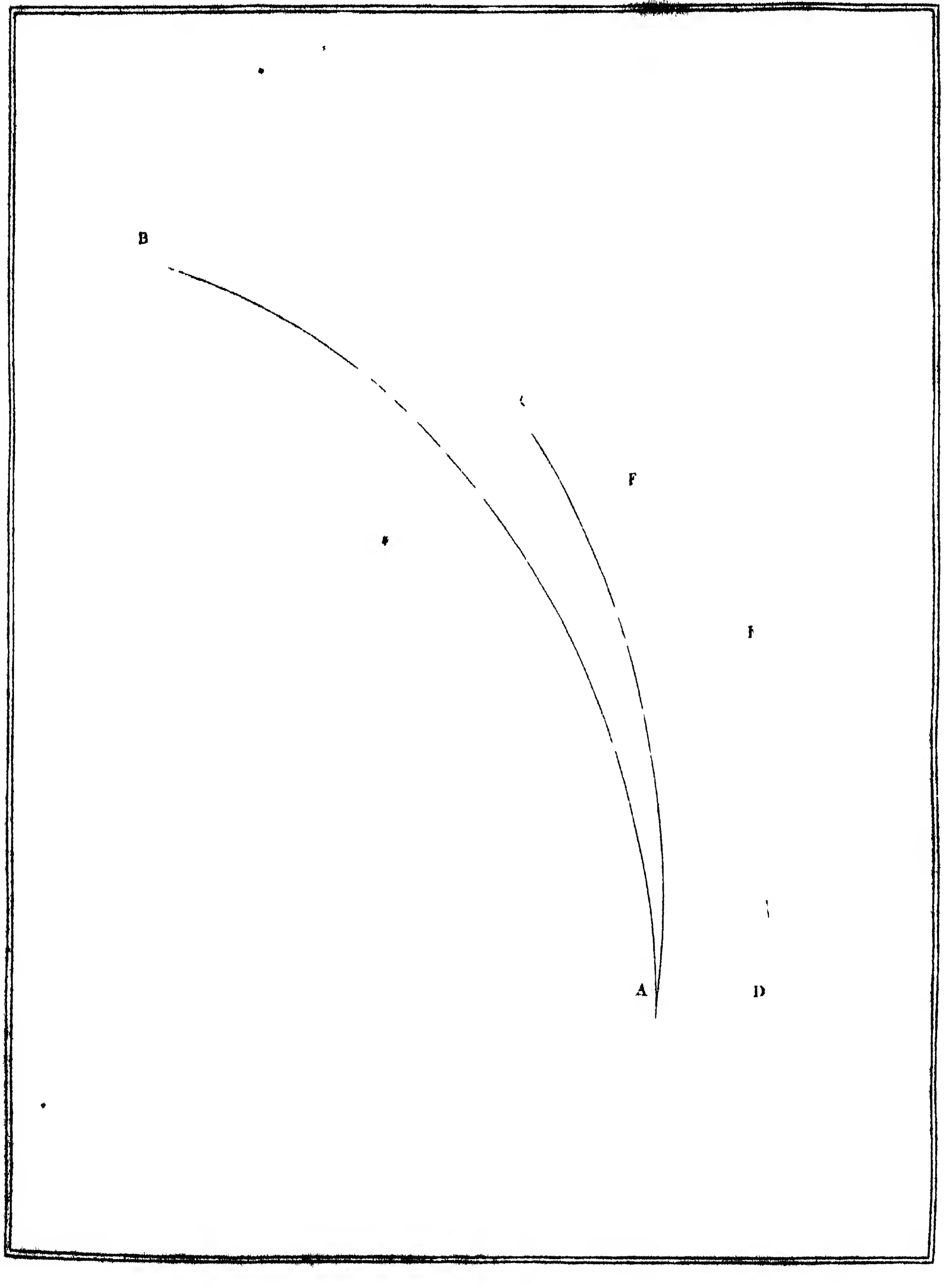
I. *An Account of two Rainbows, seen at the same Time, at* Alverstoke, Hants, July 9, 1792. *By the Rev. Mr.* Sturges. *Communicated by* William Heberden, *M. D. F. R. S.*

Read January 10, 1793.

ON the evening of the 9th of July, 1792, between seven and eight o'clock, at Alverstoke, near Gosport, on the sea coast of Hampshire, there came up, in the south-east, a cloud with a thunder-shower; while the sun shone bright, low in the horizon to the north-west.

In this shower two primary rainbows appeared, AB and AC, (Tab. I.) not concentric, but touching each other at A, in the south part of the horizon; with a secondary bow to each, DE and DF (the last very faint, but discernible), which touched likewise, at D. Both the primary were very vivid for a considerable time, and at different times nearly equally so; but the bow AB was most permanent, was a larger segment of a circle, and at last, after the other had vanished, became almost a semicircle; the sun being near setting. It was a perfect calm, and the sea was as smooth as glass.

If I might venture to offer a solution of this appearance, it would be as follows. I consider the bow AB as the true one, produced by the sun itself; and the other, AC, as produced by the reflection of the sun from the sea, which, in its perfectly smooth state, acted as a speculum. The direction of the sea, between the Isle of Wight and the land, was to the north-west, in a line with the sun, as it was then situated. The image reflected from the water, having its rays issuing from a point lower than the real sun, and in a line coming from beneath the horizon, would consequently form a bow higher than the true one, AB. And the shores, by which that narrow part of the sea is bounded, would, before the sun's actual setting, intercept its rays from the surface of the water, and cause the bow AC, which I suppose to be produced by the reflection, to disappear before the other.


B
C
F
F
A
D

II. *Description of the double horned Rhinoceros of* Sumatra. *By Mr.* William Bell, *Surgeon in the Service of the* East India *Company, at* Bencoolen. *Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read January 10, 1793.

THE animal herein described was shot, with a leaden ball from a musket, about ten miles from Fort Marlborough. I saw it the day after; it was then not in the least putrid, and I put it into the position from which the accompanying drawing was made. (See Tab. II.)

It was a male, the height at the shoulder was four feet four inches; at the sacrum nearly the same; from the tip of the nose to the end of the tail, eight feet five inches. From the appearance of its teeth and bones it was but young, and probably not near its full size.

The shape of the animal was much like that of the hog. The general colour was a brownish ash; under the belly, between the legs and folds of the skin, a dirty flesh colour.

The head much resembled that of the single horned rhinoceros. The eyes were small, of a brown colour; the *membrana nictitans* thick and strong. The skin surrounding the eyes was wrinkled. The nostrils were wide. The upper lip was pointed, and hanging over the under.

There were six *molares*, or grinders, on each side of the upper and lower jaw, becoming gradually larger backward, particularly in the upper. Two teeth in the front of each jaw.

The tongue was quite smooth.

The ears were small and pointed, lined and edged with short black hair, and situated like those of the single horned rhinoceros.

The horns were black, the larger was placed immediately above the nose, pointing upwards, and was bent a little back; it was about nine inches long. The small horn was four inches long, of a pyramidal shape, flattened a little, and placed above the eyes, rather a little more forward, standing in a line with the larger horn, immediately above it. They were both firmly attached to the skull, nor was there any appearance of joint, or muscles to move them.

The neck was thick and short, the skin on the under side thrown into folds, and these folds again wrinkled.

The body was bulky and round, and from the shoulder ran a line, or fold, as in the single horned rhinoceros, though it was but faintly marked. There were several other folds and wrinkles on the body and legs; and the whole gave rather the appearance of softness.

The legs were thick, short, and remarkably strong; the feet armed with three distinct hoofs, of a blackish colour, which surrounded half the foot, one in front, the others on each side. The soles of the feet were convex, of a light colour, and the cuticle on them not thicker than that on the foot of a man who is used to walking.

The testicles hardly appeared externally.

The penis was bent backward, and opened about eighteen inches below the anus. At its origin it was as thick as a man's leg, and about two feet and a half long; the bend in it occasions the urine to be discharged backwards. The glans is very singular: the opening of the urethra is like the mouth of a cup with its brim bending over a little, and is about three quarters of an inch in diameter; the glans here is about half an inch in diameter, and continues that thickness for an inch and a half; it is then inserted into another cup like the first, but three times as large. The glans afterwards gradually becomes thicker, and at about nine inches from the opening of the urethra are placed two bodies on the upper part of the glans, very like the nipples of a milch cow, and as large; these become turgid when the penis is erected. The whole of this is contained in the prepuce, and may be considered as glans.

From the os pubis arises a strong muscle, which soon becomes tendinous. This tendon is continued along the back, or upper part, of the penis; it is flattened, is about the size of a man's little finger, and is inserted into the upper part of the glans, near the end. The use of this muscle is to straiten the penis.

On the under side of the penis there are two muscles, antagonists to the above; they arise from the os ischium fleshy, run along the lower side of the penis, on each side of the corpus spongiosum, and are inserted fleshy into the lower side of the glans. The action of these muscles will draw in the penis, and bend it.

The male has two nipples, like the female, situated between the hind legs, they are about half an inch in length, of a pyramidal form, rounded at the end.

The whole skin of the animal is rough, and covered very thinly with short black hair. The skin was not more than one third of an inch in thickness, at the strongest part; under the belly it was hardly a quarter of an inch; any part of it might be cut through with ease, by a common dissecting knife.

The animal had not that appearance of armour which is observed in the single horned rhinoceros.

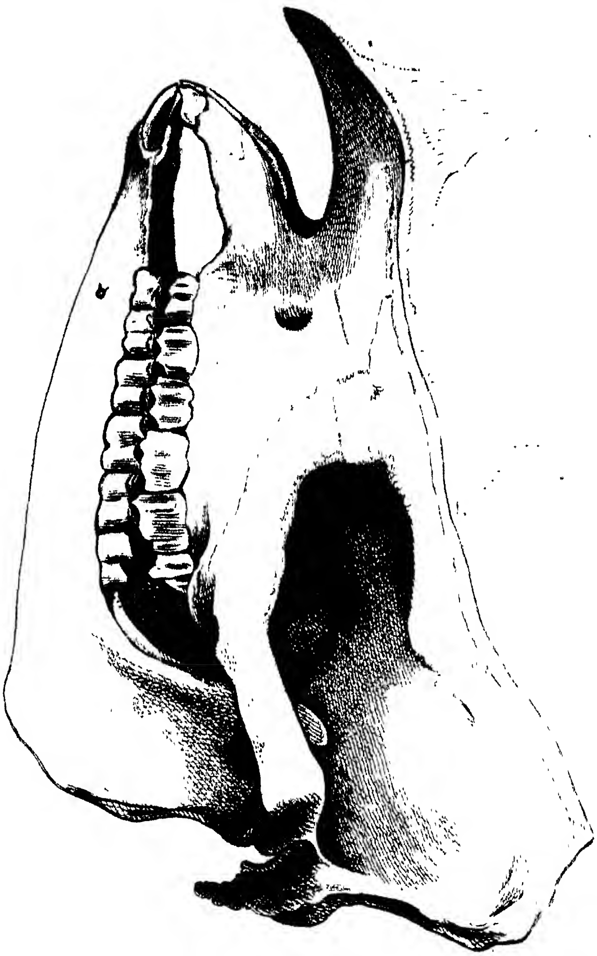
Since I dissected the male, I have had an opportunity of examining a female, which was more of a lead colour; it was younger than the male, and had not so many folds or wrinkles in its skin, of course it had still less the appearance of armour.

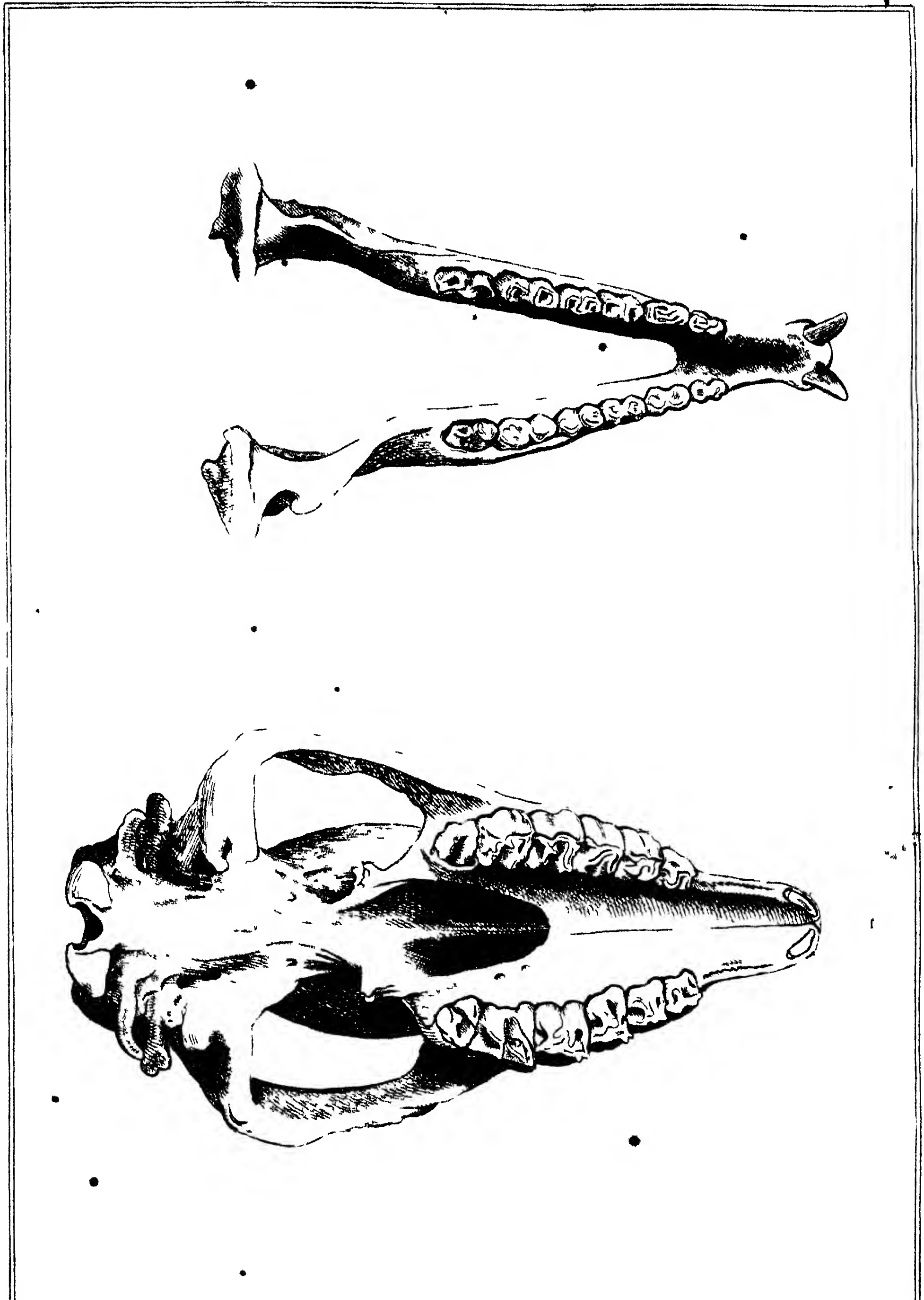
The only external mark which distinguishes it from the male is the vagina, which is close to the anus; whereas, in the male, the opening for the penis is eighteen inches below the anus.

Tab. II. Represents the entire animal.

Tab. III. The cranium.

Tab. IV. The upper and under jaw, separated from each other.





III. *Description of a Species of Chætodon, called, by the* Malays, Ecan bonna. *By Mr.* William Bell, *Surgeon in the Service of the* East India *Company, at* Bencoolen. *Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read January 17, 1793.

THE fish called *Ecan bonna*, by the Malays, is broad, flat, and of a lead colour; the belly is flat, white, and in places tinged with green. The eyes are a bright yellow. The body is covered with small semicircular scales.

Its length is generally about eighteen inches; its breadth thirteen, and, at the thickest part, it is nearly three inches thick.

It is frequently caught at Bencoolen, and several other parts on the west coasts of Sumatra, and is said to grow to a much larger size. Its flesh is white, firm, and well flavoured, and it is considered as a good fish for the table.

It has six fins: two pectoral, two ventral, one dorsal, and one anal fin. The tail is broad, and of a triangular form.

The pectoral fins are small, blunted at their ends, and placed a little behind the gills.

The ventral fins are placed on the sternum, and are longer, and more pointed.

The dorsal fin arises at the beginning of the spinous processes of the back, and is continued down nearly to the tail.

The anal fin arises a little below the anus, and is also continued on almost to the tail. It is strong and broad, like the dorsal, and projects a little farther backward than it.

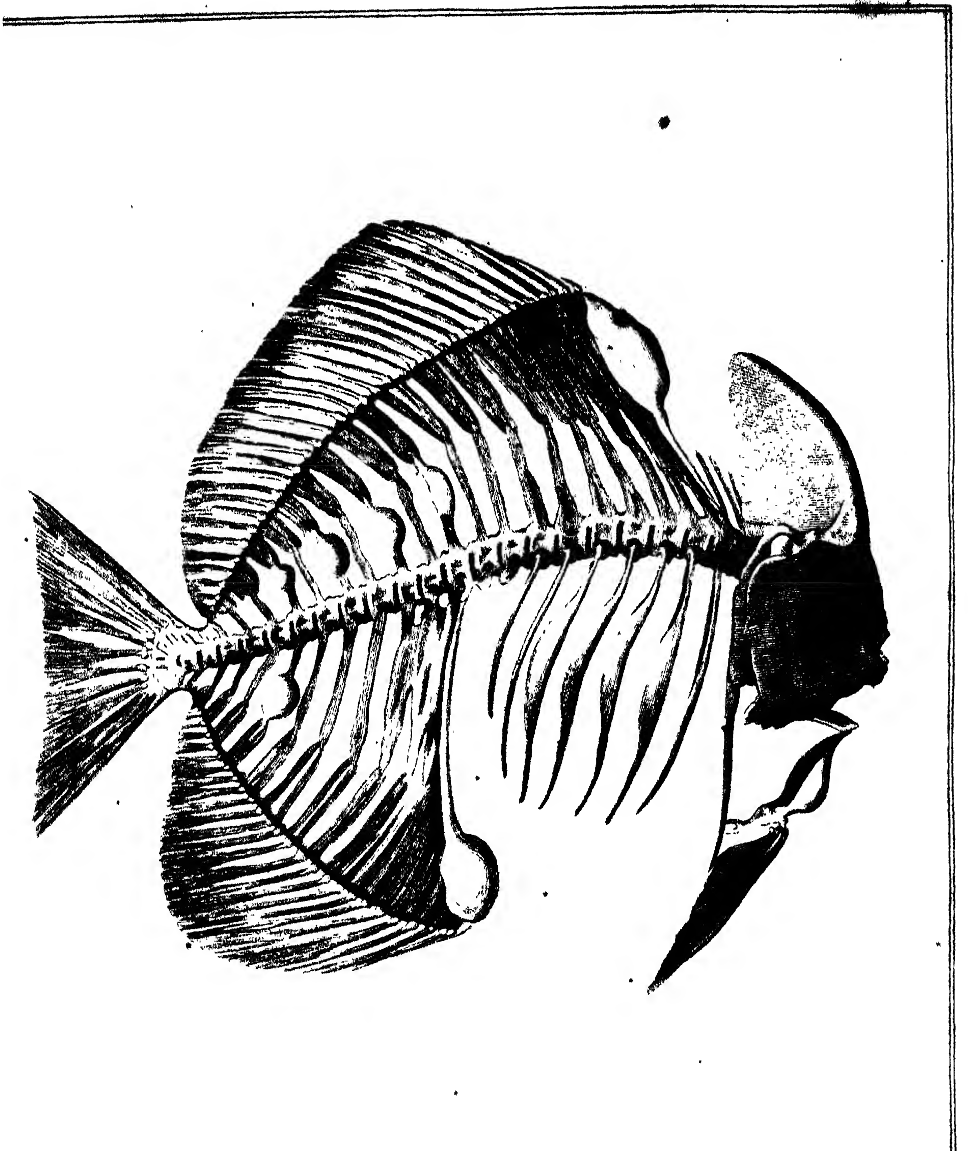
The mouth is small, and each jaw contains five rows of small teeth, about the thickness of hog's bristles, and of equal thickness throughout their length. The grinding, or cutting surfaces of the front, second, and third rows, in both jaws, are divided into three points. The two inner rows are pointed, and bent a little backward.

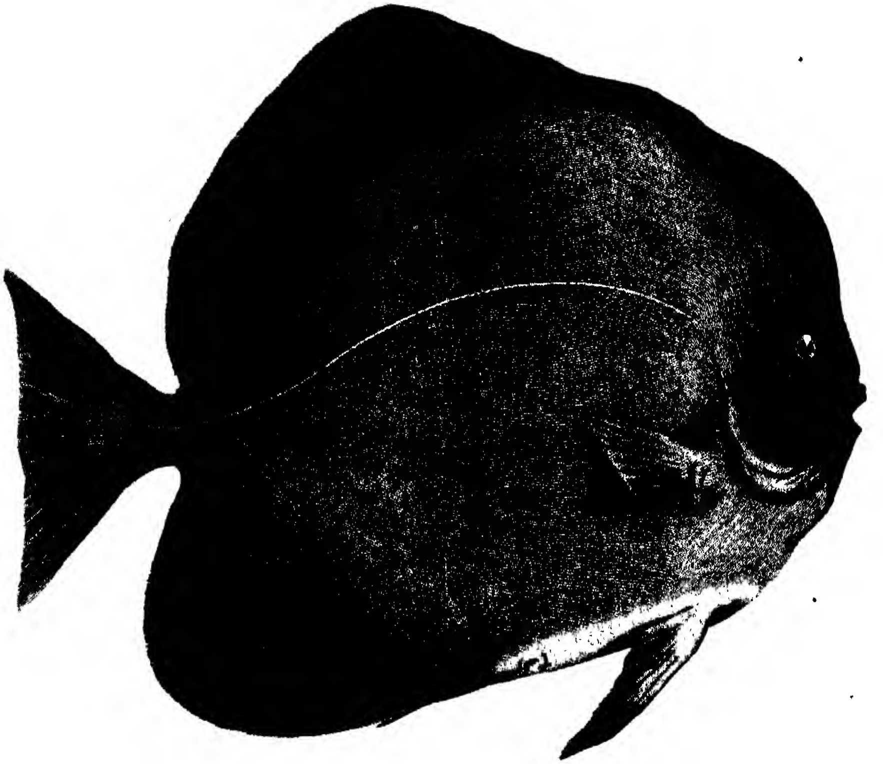
The stomach was empty, so that I had not an opportunity of ascertaining its food. The intestinal canal was long, like that of fish which feed on vegetables; and the œsophagus was thick set with pyramidal bodies, like the œsophagus of the turtle.

The skeleton is very singular, many of the bones having tumours, which, in the first fish I saw, I supposed to be exostoses arising from disease; but on dissecting a second, found the corresponding bones had exactly the same tumours, and the fishermen informed me they were always found in this fish; I therefore conclude them to be natural to it.

In Mr. HUNTER'S collection are two or three of these bones, but I never knew what fish they belonged to; they were supposed to be from the back of some of the large rays.

What advantage can arise from these large tumours is difficult to say. Those on the spines of the vertebræ seem to answer no evident purpose, nor those at the origin of the dorsal, and anal fins. The particular form of the sternum, to which the ventral fins are joined, seems to be intended to give greater surface for the attachment of the muscles, and to increase their action.





These tumours are spongy, and so soft as to be easily cut with a knife; they were filled with oil.

The air-bladder is very large, for the size of the fish, probably to counteract the weight of the bony matter in the skeleton.

It is generally caught near the shore, where there are sea-weeds, and the Malays say it is a dull swimmer.

Tab. V. Represents the fish herein described.

Tab. VI. The skeleton of the same.

IV. *Account of some Discoveries made by Mr.* Galvani, *of* Bologna; *with Experiments and Observations on them. In two Letters from Mr.* Alexander Volta, *F. R. S. Professor of Natural Philosophy in the University of* Pavia, *to Mr.* Tiberius Cavallo, *F. R. S.*

Read January 31, 1793.

LE sujet des découvertes et des recherches, dont je vais vous entretenir, Monsieur, est *l'Electricité Animale*; sujet qui doit vous interesser vivement. Je ne sais si vous avez encore vu l'ouvrage d'un professeur de Bologna, Mr. GALVANI, qui a paru il y a à-peu-près un an, avec ce titre; ALOYSII GALVANI *de Viribus Electricitatis in Motu Musculari Commentarius. Bononiæ,* 1791, *in 4to,* de 58 pages, avec quatre grandes planches; ou du moins si vous en avez eu notice. Il contient une des plus belles et des plus surprenantes découvertes, et le germe de plusieurs autres. Nos journaux Italiens en ont donné différents extraits, entre autres celui du Dr. BRUGNATELLI de Pavie, qui a pour titre, *Giornale Fisico-medico*; auquel j'ai fourni moi-même deux longs memoires, qui seront suivis de quelques autres, ayant beaucoup étendu les experiences, et poussé les recherches plus loin sur ce sujet.

Or c'est une esquisse, tant de la découverte admirable de Mr. GALVANI, que des progrès que j'ai été assez heureux de faire dans cette nouvelle carrière, que je vais vous tracer, Monsieur, dans cet écrit, que je souhaite que vous presentiez au digne President de la Societé Royale, le Chevalier Banks, pour être communiqué, s'il le croit à propos, à cette savante Compagnie, comme un foible temoignage de ma reconnoissance pour l'honneur qu'elle m'a fait de m'associer à son corps, et de mon zele et empressement à répondre à son invitation de lui faire part, de tems à autre, du fruit de mes recherches.

(1.) Le Dr. Galvani ayant coupé et préparé une grenouille, de maniere que les jambes tenoient à une partie de l'épine du dos, tronquée du reste du corps, uniquement par les nerfs cruraux mis à nud, vit qu'il s'excitoit des mouvements très vifs dans ces jambes, avec des contractions spasmodiques dans tous les muscles, chaque fois que (ce reste d'animal, se trouvant placé à une distance considerable du grand conducteur de la machine électrique, et dans certaines circonstances, que j'expliquerai ensuite,) on tiroit de ce même conducteur, non pas sur le corps de l'animal, mais sur tout autre corps, et dans toute autre direction, une étincelle. Les circonstances requises etoient donc, que l'animal, ainsi dissequé, se trouvât en contact, ou très près de quelque métal, ou autre bon conducteur assez étendu, et mieux encore entre deux semblables conducteurs, dont l'un étoit tourné vers l'extremité des dites jambes, ou quelqu'un de ses muscles, l'autre vers l'épine, ou les nerfs: il étoit aussi très avantageux, qu'un de ces conducteurs, que l'auteur distingue par le nom de *conducteur des nerfs*, et de *conducteur des muscles*, et préferablement ce dernier, eût une libre communication avec le plancher. C'est dans cette position surtout, que les jambes de la grenouille préparée, comme on a dit, recevoient de violentes secousses, s'élançoient et se debattoient avec vivacité à chaque étincelle du conducteur de la machine, quoiqu'il fut assez éloigné, et quoique la décharge ne se fit, ni sur le conducteur des nerfs, ni sur celui des muscles, mais sur un autre quelconque, pareillement éloigné d'eux, et ayant tout autre communication par où transmettre une telle décharge, par exemple, sur une personne placée à l'angle opposé de la chambre.

(2.) Ce phénomene étonna Mr. GALVANI, peut-être plus qu'il n'auroit dû faire ; car enfin le pouvoir, non seulement des étincelles électriques lorsqu'elles frappent immédiatement les muscles ou les nerfs d'un animal, mais d'un courant de ce fluide qui les traverse, de quelque manière que ce soît, avec une suffisante rapidité, son grand pouvoir, dis-je, d'y exciter des commotions, étoît une chose assez connue ; d'ailleurs il étoit visible comment, dans cette expérience, et dans toutes celles du même genre rapportées dans la première et seconde partie de son ouvrage, et qui sont représentées dans les deux premieres planches de figures, sa grenouille se trouvoit effectivement exposée à être traversée par un tel courant. On n'a qu'à se retracer l'action très-connue des atmospheres électriques, ou ce qu'on appelle *electricité de pression* ; par laquelle le fluide des corps *déférents*, plongés dans la sphere d'activité d'un corps électrisé quelconque, est poussé et deplacé, en raison de la force, et de l'étendu, de cette sphere, et entretenu en cet état de déplacement tant que l'électricité dans le corps dominant subsiste, laquelle otée il revient à sa place des endroits éloignés, peu-à-peu si elle se dissipe petit à petit, et en un instant si on la détruit instantanément, en déchargeant tout d'un coup le corps qui en est revêtu. C'est donc ce *courant de retour*, ce reflux de fluide électrique dans les corps *déférents* contigus à la grenouille, ou proches d'elle, son passage brusque du *conducteur des muscles* au *conducteur des* *nerfs*, ou *vice versa*, à travers son corps, surtout lorsqu'un tel courant est resserré dans le canal unique et etroit des nerfs, qui excite les spasmes et les mouvements dans les expériences dont il est ici question. Mr. GALVANI, qui semble n'avoir pas assez réfléchi à cette action des atmospheres électriques, et qui ne connoissoit pas encore la prodigieuse sensibilité de sa grenouille, singulierement préparée de la maniere susdite, (je dirai ici, que je l'ai trouvée à-peu-près égale dans tous les autres petits animaux, comme lezards, salamandres, souris) fut extremement frappé d'un tel effet, qui ne paroitra pas si merveilleux à d'autres physiciens. Ce fut pourtant le premier pas, qui le conduisit à la belle et grande découverte d'une *électricité animale* proprement dite, appartenante non seulement aux grenouilles, et à d'autres animaux à sang froid, mais aussi-bien à tous les animaux à sang chaud, quadrupedes, oiseaux, &c.; découverte qui fait le sujet de la troisieme partie de son ouvrage, sujet absolument neuf, et très interessant. C'est ainsi qu'il nous a ouvert un champ très vaste, dans lequel nous nous proposons d'entrer, et de poursuivre les recherches, après que nous nous serons arreté encore un peu sur ces experiences préliminaires qui concernent l'action de l'électricité artificielle, ou étrangere, sur les fibres nerveuses et musculaires.

(3.) Ce fut le hazard qui presenta à Mr. GALVANI le phénomene que nous venons de décrire, et dont il fut étonné, je le répéte, plus qu'il n'auroit du être. Cependant qui est ce qui auroit cru, qu'un courant électrique, foible au point de ne pouvoir être rendu sensible par les électrometres les plus delicats, fut capable d'affecter si puissamment les organes d'un animal, et d'exciter dans ses membres, découpés une ou plusieurs heures avant, des mouvements, tels que l'animal vivant n'en produit pas de plus forts, comme d'élancer vigoureusement les jambes, de sauter, &c. pour ne rien dire des convulsions toniques les plus violentes ? Or tel est le courant qui envahit le petit animal couché, par exemple, sur la table, auprès de quelque métal, ou entre deux bons conducteurs non isolés, lorsqu'une personne tire du grand conducteur électrique, suspendu plusieurs pieds au dessus, une médiocre étincelle, et dirige la décharge par une tout autre voie.

(4.) Je dis *médiocre* ; car si elle est bien forte, et si la distance de ce conducteur, puissamment électrisé et volumineux, aux corps posés sur la table n'est pas fort grande, il paroîtra des petites étincelles dans les interstices de ces corps surtout métalliques, et là méme ou la grenouille fait un anneau de communication entr'eux ; étincelles produites evidemment par ce fluide électrique de retour, dont nous avons parlé ci-dessus. (sect. 2.) Ou, si la chose n'arrive pas à ce point, au lieu des étincelles, on pourra observer des mouvements assez marqués de quelques électrometres placés sur la même table, et aux mêmes endroits. Or dans ce cas, où les électrometres donnent des signes, et beaucoup plus dans l'autre, où l'on obtient les susdites étincelles, on pourra observer que même une grenouille entiere et intacte, où un autre petit animal quelconque, un lezard, un souris, un moineau, sont saisis de fortes convulsions dans tous leurs membres, surtout dans les jambes, qui s'élancent avec vivacité, si le passage du fluide électrique (le courant de retour) suit la direction de ces mêmes jambes d'un bout à l'autre. Jusques-là point de merveille ; la surprise est dans le cas où le courant électrique n'étant plus sensible, pas même aux électrometres les plus delicats, il excite encore les mêmes convulsions, les mêmes mouvements et debats, si non dans la grenouille entiere, au moins dans ses membres disséqués et préparés à la manière de Mr. GALVANI.

(5.) Je me suis appliqué, avec quelque attention, à determiner quelle étoit la moindre force électrique requise à produire ces effets, aussi bien dans une grenouille intacte et pleine de vie, que dans une disséquée et préparée à la dite maniere; ce que Mr. GALVANI avoit omis de faire. J'ai choisi la grenouille de préference à tout autre animal, à cause qu'elle est douée d'une vitalité très-durable, et qu'il est fort aisé de la préparer. Au reste j'ai aussi fait des épreuves sur d'autres petits animaux, dans cette vue, et avec un succès à-peu-près égal. Pour bien évaluer la force du courant électrique, j'ai cru devoir soumettre l'animal destiné aux expériences de ce genre, non pas aux courants de retour occasionnés par les atmospheres, (sect. 2.) mais aux décharges électriques directes, tantôt d'un simple conducteur, tantôt d'une bouteille de Leyde, et en sorte que tout le courant dût traverser le corps de l'animal. A cet effet j'avois soin de le tenir isolé d'une maniere ou de l'autre, et le plus souvent en l'attachant, par des epingles, à deux plateaux de bois tendre, portés par des colonnes de verre.

(6.) J'ai donc trouvé, que pour la grenouille vivante et entiere il suffisoit de l'électricité d'un simple conducteur, de moyenne grandeur, quand elle arrivoit seulement à pouvoir donner une très foible étincelle, et à élever de cinq à six degrés l'électrometre de HENLY. Que si je me servois d'une bouteille de Leyde, aussi de moyenne grandeur, une charge de celle-ci beaucoup plus foible produisoit l'effet, telle, par exemple, que ne donnant pas la moindre étincelle, et n'étant aucunement sensible au quadrant-électrometre, l'étoit à peine à un électrometre de CAVALLO au point d'écarter d'une ligne environ ses petits pendules.

(7.) Cela, comme je viens de montrer, pour une grenouille entiere et intacte; car pour une disséquée et préparée en différentes manières, et sur-tout à la façon de GALVANI, où les jambes tiennent à l'épine dorsale par les seuls nerfs cruraux, une électricité beaucoup plus foible encore, soit du conducteur, soit de la bouteille de Leyde, (le fluide étant obligé d'enfiler ce passage étroit des nerfs,) ne manquoit pas d'exciter les convulsions, &c. Oui une électricité quarante ou cinquante fois plus foible, comme une charge de la bouteille absolument imperceptible au dit électrometre de CAVALLO, et même à celui extremement delicat de BENNET; une charge, que je ne pouvois rendre sensible qu'à l'aide de mon *condensateur,* et que je crois pouvoir évaluer à cinq ou six centiemes de degré de l'électrometre de CAVALLO.

(8.) Voila donc, dans les jambes de la grenouille attachées à l'épine du dos uniquement par ses nerfs bien dépouillés, une nouvelle espèce d'électrometre; puisque des charges électriques qui, ne donnant aucun signe à ceux-ci, paroitroient nulles, en donnent de si marqués par ce nouveau moyen, par un tel *électrometre animal,* si on peut l'appeller ainsi.

(9.) Lorsqu'on a vu comment une grenouille ainsi préparée se ressent, et est saisie de fortes convulsions par une électricité extremement foible, par un courant de fluide imperceptible, on ne doit surement plus être surpris, qu'elle se debatte de même lorsqu'un corps quelconque décharge tout d'un coup le grand conducteur de la machine électrique, et fait qu'un autre courant de fluide électrique, grand ou petit, du fluide ci-devant déplacé dans les corps *déférents* auprès de la grenouille, et qui se rétablit, comme on a expliqué plus haut, (sect. 2) passe rapidement à travers ses nerfs. Supposons que ce courant de retour soit à peine équivalent à celui que lance directement un conducteur suffisamment volumineux, avec une électricité non étincellante, et presqu' insensible jusqu'à l'électrometre de CAVALLO, ou une petite bouteille de Leyde, chargée à peine un dixieme de degré de ce même électrometre ; supposons, dis-je, que le courant électrique ne soit pas plus fort que cela, il suffit encore, comme mes expériences, rapportées ci-dessus, (sect. 6. et 7.) font voir, pour exciter les mouvements dont il s'agit.

(10.) Mais si on ne doit plus être surpris, après ces expériences, de celles de Mr. GALVANI décrites dans la première et seconde partie de son ouvrage, comment s'empecher de l'être de celles tout-à-fait nouvelles et merveilleuses qu'il rapporte dans la troisième ? Par lesquelles il obtint les mêmes convulsions et mouvements violents des membres, sans avoir recours à aucune électricité artificielle, ou excitation étrangere, par la seule application d'un *arc conducteur* quelconque, dont un bout touchât aux muscles, et l'autre aux nerfs ou à l'epine de la grenouille, preparée de la manière decrite. Cet arc conducteur pouvoit être ou entièrement métallique, ou partie métallique partie d'autres corps de la classe des *déférents*, comme d'eau, d'une ou plusieurs personnes, &c. Même les bois, les murailles, le plancher, pouvoient entrer dans le circuit, pourvu qu'ils ne fussent pas trop secs ; il n'y avoit que l'interposition des corps *cobibents*, comme verre, resines, soye, qui empechât l'effet. Les mauvais conducteurs cependant ne servoient pas si bien, et seulement pour les premiers moments après la préparation de la grenouille, tant que les forces vitales se soutenoient en pleine vigueur; après quoi il n'y avoit plus que les bons conducteurs qu'on pût employer avec succès, et bientôt on ne pouvoit réussir qu'avec les excellents, c'est à dire, avec des arcs conducteurs entièrement métalliques. Il trouva au surplus un grand avantage à appliquer une espèce d'armure métallique à cette portion d'épine qu'il laissoit attachée aux nerfs cruraux, et aux nerfs eux-mêmes, et surtout à revêtir cette partie d'une feuille mince d'étain ou de plomb.

(11.) Mr. GALVANI ne s'arreta pas, dans ces expériences vraiment étonnantes, aux grenouilles; il les étendit avec succès, non seulement à plusieurs autres animaux à sang-froid, mais aussi aux quadrupedes, et aux oiseaux; dans lesquels il obtint les mêmes resultats, moyennant les mêmes préparations; qui consistoient à degager de ses enveloppes un des principaux nerfs, là où il s'implante dans un membre susceptible de mouvement, à armer ce nerf de quelque lame ou feuille métallique, et à etablir une communication, à l'aide d'un arc conducteur, de cette armure du nerf aux muscles dépendants.

(12.) C'est ainsi qu'il découvrit heureusement, et nous demontra, de la manière la plus evidente, l'existence d'une veritable *électricité animale* dans tous, ou presque tous les animaux. Il paroit prouvé en effet par ses expériences, que le fluide électrique tend sans cesse à passer d'une partie à l'autre du corp organique vivant, et même des membres tronqués, tant qu'il y subsiste un reste de vitalité; qu'il tend à passer des nerfs aux muscles, ou *vice versa,* et que les mouvements musculaires sont dûs à une semblable transfusion, plus ou moins rapide. En verité il semble qu'on ne peut rien opposer à cela, ni à la façon dont Mr. GALVANI explique la chose, par une espèce de décharge semblable à celle de la bouteille de Leyde. Cependant un grand nombre de nouvelles expériences, que j'ai faites sur ce sujet, font voir qu'il y a bien des restrictions à faire, tant à la chose, qu'aux consequençes que l'auteur en a tirées; en même tems qu'elles étendent beaucoup les phénomenes attribués à cette *électricité animale*, et nous la representent dans un grand nombre de circoustances et de combinaisons nouvelles.

(13.) Mr. GALVANI, suivant l'idée qu'il s'est fait, d'après ses expériences, et pour suivre, en tout point, l'analogie de la bouteille de Leyde et de l'arc conducteur, prétend qu'il y ait naturellement un excès de fluide électrique dans le nerf, ou dans l'interieur du muscle, et un defaut correspondant dans l'exterieur, ou *vice versa*; et suppose conséquemment qu'un bout de cet arc doit communiquer au nerf, qu'il regarde comme le fil conducteur, ou crochet de la bouteille; l'autre bout à la face exterieure du muscle. Toutes les figures de la troisième et quatrième planche, et toutes ses explications reviennent à cela. Mais s'il avoit un peu plus varié les expériences, comme j'ai fait, il auroit vu que ce double contact du nerf et du muscle, ce circuit qu'il imagine, n'est pas toujours nécessaire. Il auroit trouvé, ce que j'ai trouvé, qu'on peut exciter les mêmes convulsions, les mêmes mouvements, dans les jambes, et autres membres des grenouilles, et de tout autre animal, par des attouchements métalliques, soit à deux parties du nerf seul, soit à deux muscles, et même à differents points d'un seul et simple muscle.

(14.) Il est vrai qu'on ne reussit pas, à beaucoup près, si bien de cette manière que de l'autre, et qu'il faut, dans ce cas, avoir recours à un artifice, dont nous aurons occasion de parler plus au long, et qui consiste à employer deux métaux differents ; artifice qui n'est pas absolument necessaire lorsqu'on expérimente suivant le procédé de GALVANI, decrit ci-dessus, (sect. 10. et 11.) du moins tant que la vitalité dans l'animal, ou dans ses membres coupés, se soutient en pleine vigueur ; mais enfin, puisque avec des armures de differents métaux appliquées, soit aux nerfs seuls, soit aux seuls muscles, on vient à bout d'exciter les contractions dans ceux-ci, et les mouvements des membres, on doit conclure que s'il y a des cas (ce qui pourroit bien encore paroitre douteux) où la pretendue décharge entre nerf et muscle (sect. 12. et 13.) est cause des mouvements musculaires, il y a bien aussi des circonstances, et plus fréquentes, où l'on obtient les mêmes mouvements, par un tout autre jeu, par une tout autre circulation, du fluide électrique.

(15.) Oui c'est un tout autre jeu du fluide électrique, dont on doit dire plutôt qu'on trouble l'equilibre, que de le retablir, en ce qu'il coule d'une partie à l'autre du nerf, du muscle, &c. tant interieurement par leurs fibres conductrices, qu'extérieurement par la voie des conducteurs métalliques appliquées, non pas en conséquence d'un excès ou defaut respectif, mais par une action propre de ces mêmes métaux, lorsque ceux-ci sont de differente espece. C'est ainsi que j'ai découvert une nouvelle loi, qui n'est pas tant une loi d'électricité animale, qu'une loi d'électricité commune ; à laquelle on doit attribuer la plûpart des phénomenes, qui paroissoient, d'après les expériences de GALVANI, et d'après plusieurs autres que j'avois faites moi-même à la suite de celles-là, appartenir à une veritable électricité animale spontanée, et qui n'en sont pas ; ce sont réellement des effets d'une électricité artificielle très foible, qui s'excite d'une manière dont on ne s'étoit pas douté, par la simple application de deux armures de differents métaux, comme j'ai deja indiqué, et que j'expliquerai mieux ailleurs.

(16.) Je dois dire ici, qu'à la découverte de cette loi nouvelle, de cette électricité artificielle jusqu'à présent inconnue, je me defiai d'abord de tout ce qui m'avoit paru demontrer une électricité animale naturelle, dans le sens propre, et que j'etois sur le point de revenir de cette idée. Mais repassant, avec un examen reflechi, tous les phénomenes, et repetant les expériences sous ce nouveau point de vue, je trouvai enfin que quelques unes soutiennent encore cet examen, (celles, par exemple, où l'on n'a pas besoin d'armures differentes, ni même d'armure quelconque, un simple fil métallique, ou tout autre corps déférent, faisant office d'arc conducteur entre le nerf isolé et un des muscles dépendants, pouvant exciter dans ceux-ci les convulsions), (sect. 10, &c.) et qu'ainsi l'électricité animale naturelle et proprement organique subsiste, et ne peut pas être renversée entièrement. Les phénomenes qui l'établissent, quoique beaucoup plus limités, ne laissent pas que d'être demonstratifs, comme je viens d'indiquer, et comme on verra mieux dans la suite.

(17.) Ce qu'on trouvera peut-être plus desagréable, c'est qu'il faut aussi renfermer en des limites plus étroits son empire dans l'économie animale, et renoncer aux plus belles idées qu'on avoit conçues, et qui paroissoient nous mener à expliquer clairement tous les mouvements des muscles. Mes expériences, variées de toutes les manières possibles, montrent que le mouvement du fluide électrique, excité dans les organes, n'agit point immédiatement sur les muscles; qu'il ne fait qu'exciter les nerfs, et que ceux-ci, mis en action, excitent à leur tour les muscles. Quelle soit cette action des nerfs; comment elle se propage d'une de ses parties aux autres; comment elle passe aux muscles, et comment il en resulte le mouvement de ces derniers; ce sont encore des problemes, pour l'explication desquels nous n'en sommes pas plus avancés qu'avant la découverte dont il s'agit.

(18.) Je viens maintenant aux expériences qui prouvent toutes les assertions que j'ai avancées dans ces derniers paragraphes. Dans la foule qui se présente j'en choisirai quelques unes seulement, celles qui me paroissent mieux établir certains principes, la plûpart nouveaux et differents de ceux adoptés par Mr. GALVANI. Mais disons premièrement encore un mot des expériences de cet auteur. Je ne sais s'il en a fait d'autres, mais celles dont il nous rend compte dans son ouvrage sont renfermées dans un cercle trop étroit; il s'agit toujours de découvrir et isoler les nerfs, et d'établir une communication, de corps conducteurs de l'électricité, entre ces nerfs et les muscles qui en dépendent; (comme on voit dans toutes les figures des quatres planches jointes à ce même ouvrage) lorsqu'on se propose d'exciter les convulsions et mouvements de ces muscles, par l'action du fluide électrique. Il suppose donc, dans tous les cas, et il s'explique la dessus assez clairement, que la transfusion du fluide électrique produite, soit par l'électricité artificielle, soit par l'électricité animale naturelle, doive se faire des nerfs aux muscles, ou *vice versa*; que ces deux termes au moins y doivent être compris, pour que les mouvements musculaires ayent lieu; et vraiment toutes les expériences qu'il nous décrit semblent prouver cela. Mais c'est qu'elles roulent, comme j'ai déja dit, dans ce cercle trop étroit, dont il n'est jamais, ou presque jamais, sorti. En variant les expériences de ce genre de plusieurs manières, j'ai fait voir, que ni l'une ni l'autre de ces conditions, savoir, de découvrir et isoler les nerfs, et de toucher simultanément ceux-ci et les muscles, pour procurer la prétendue décharge, sont absolument necéssaires (sect 13.). Il suffit, lorsqu'on a par exemple découvert le nerf ischiatique.à un chien, à un agneau, &c. de faire passer un courant électrique d'une partie de ce nerf à une autre, même prochaine, en laissant tout le reste intact et libre, et intacte encore plus toute la jambe ; il suffit, dis-je, de cela pour voir excités dans cette jambe les convulsions et les mouvements les plus forts ; et cela, soit qu'on employe une électricité artificiélle étrangere, soit qu'on mette en mouvement le fluide électrique inhérent au nerf lui-même. Voici de quelle manière je fais ces expériences.

(19.) EXBERIENCE A. Je serre, avec des pincettes, le nerf ischiatique un peu au dessus de son insertion dans la cuisse, et j'applique, quelques lignes plus haut, une piece de monnoye, ou une autre lame métallique, sur ce même nerf, detaché soigneusement de ses adhérences, et soutenu par un fil, ou appuyé à une plaque de verre, à un bâton de cire d'Espagne, ou de bois sec, ou à tout autre corps mauvais conducteur. Alors appuyant le ventre d'une bouteille de Leyde, très foiblement chargée, aux dites pincettes, je porte le crochet en contact de l'autre lame métallique ; et voila que la décharge qui se fait, quand même elle n'est pas assez forte pour donner la moindre étincelle, fait entrer en convulsion tous les muscles de la cuisse et de la jambe, qui est secouée et s'élance avec plus ou moins d'impetuosité. Et cependant toute cette jambe, et une partie même du nerf qui en deborde, se trouvoient, comme on voit, hors la route que le fluide électrique a parcouru dans son trajet, de sorte qu'une petite partie seulement du nerf a pu être irritée ; cela néanmoins a suffi pour occasionner la contraction des muscles.

(20.) EXPERIENCE B. Il en est de même, c'est-à-dire de semblables convulsions et mouvements de la jambe ont lieu, sans avoir recours à une électricité étrangere, par la décharge qui se fait, en certaine maniere naturellement, lorsqu'ayant appliqué, comme ci-dessus, les mêmes pincettes, ou une lame d'argent, à une partie du nerf, et une lame de tout autre métal, et surtout d'étain ou de plomb, à une autre partie, on les fait simplement communiquer entr'elles, soit par un contact immédiat, ou par l'entremise d'une troisième piece de métal, qui fasse l'office d'arc conducteur.

(21.) Or donc voila les mêmes effets, des convulsions et mouvements musculaires les plus vifs, sans que la décharge de fluide électrique se fasse entre les nerfs et les muscles, comme Mr. GALVANI suppose toujours ; et sans qu'il soit besoin qu'un bout de l'arc conducteur communique aux uns, et l'autre bout aux autres. Mais aussi l'autre condition, de dépouiller un nerf quelconque et le mettre à nud, n'est pas plus requise, comme les expériences suivantes vont montrer.

EXPERIENCE C. J'applique les armures, ou lames de différents métaux (c'est cette difference des armures qui est essentielle) (sect. 14. et 15.) à une grenouille toute entière et vivante, revetue même de sa peau, en un mot intacte : je colle, par exemple, une feuille mince d'étain sur son dos, ou sur les reins, et je pose une piece de monnoye d'argent sous ses cuisses, ou sous son ventre, l'y comprimant un peu ; cela fait, j'avance cette monnoye, en la glissant, jusqu'au contact de la feuille d'étain, ou bien j'établis une communication entre ces deux armures, moyennant un fil d'archal, ou une autre piece de métal quelconque ; et voila qu'il s'excite des convulsions spasmodiques dans tous les muscles du ventre, des cuisses, du dos, avec de violentes secousses des jambes, une contraction et courbure de l'épine, &c. lesquelles convulsions et spasmes, quoique presqu'universelles, sont cependant plus marqués dans les membres et muscles qui touchent, ou avoisinent, les armures, et plus encore dans ceux qui dépendent des principaux nerfs proches eux-mêmes aux dites armures.

(22.) Ces expériences reussissent dans quelques autres animaux ; dans les poissons, et dans les anguilles sur-tout, auxquels il n'est pas nécessaire d'ôter la peau, quoiqu'elle ne laisse pas que d'empécher un peu l'action. C'est pourquoi en la leur otant, au moins en partie, particulièrement à la grenouille, on obtient plus surement les effets, et on les obtient beaucoup plus grands. On gagne encore, à cet égard, si on coupe la tête à la grenouille, et si on finit de la tuer en lui enfonçant une grosse épingle dans la moelle épiniere ; on excite alors, par le moyen décrit des armures métalliques différentes, des mouvements plus forts, ou qui paroissent au moins plus marqués, parce qu'ils ne se confondent pas avec les autres mouvements que l'animal se donne étant en vie.

(23.) S'il est avantageux, comme on vient de voir, d'ôter la peau aux grenouilles, quoique fort mince et assez humide, il l'est beaucoup plus, et même nécessaire, de l'ôter à presque tous les autres animaux, lezards, salamandres, serpents, tortues, et sur-tout aux quadrupedes, et aux oiseaux, fournis d'une peau plus seche, et beaucoup plus épaisse, pour reussir dans ces expériences. Voici donc comment je m'y prends.

Experience D. J'attache à une table, au moyen de quelques grosses épingles, un lezard, une souris, un poulet, &c. et en faisant une incision à la peau et aux autres intéguments, jusqu'à la chair nue, sur le dos de l'animal ainsi assujetti, je renverse les intéguments des deux cotés; j'en fais autant à la cuisse ou à la jambe, après quoi j'applique les deux armures aux endroits dénués, ici la feuille d'étain, là la cuiller ou la piece de monnoye. Alors, toutes les fois que je fais communiquer entr'elles ces deux armures, il s'excite de fortes contractions dans les muscles adjacents, et sur-tout dans ceux de la cuisse et de la jambe, qui remue et se debat très-fort. Ces secousses sont beaucoup plus violentes selon que la feuille d'étain se trouve appliquée plus près du nerf ischiatique, et la lame d'argent mieux appliquée au muscle qu'on appelle *gluteus,* ou à l'autre dit *gastrocnemius,* et toujours plus si on va jusqu'à découvrir ce même nerf, et a le revêtir lui-même de la feuille d'étain; si, le laissant attaché seulement aux muscles dans lesquels il s'implante, on lui ôte tout autre adherence; si enfin on détache tout le membre du reste du corps, avec son nerf pendant, et on l'assujettit seul aux expériences.

Je suis, &c.

A. VOLTA.

Septembre 13, 1792.

Second Letter.

(24.) AU reste on comprend bien que ce que je viens de faire observer, par rapport aù nerf ischiatique et à la jambe, a lieu pour le nerf brachial et le bras, et pour tout autre nerf relativement aux muscles et membres régis par ces nerfs.

(25.) Ces dernières préparations reviennent à celles de Mr. GALVANI ; et elles prouvent bien qu'il est avantageux de mettre à découvert les nerfs, et plus encore de les détacher tout-au-tour ; mais nullement que ce soit une condition nécessaire, puisqu'on ne manque pas d'obtenir les mémes convulsions et mouvements des membres lorsqu'on découvre simplement les muscles, et qu'on laisse tous les nerfs enveloppés et cachés sous eux dans l'état naturel, comme toutes mes autres expériences ci-devant rapportées (sect. 21. 22. 23.) font voir.

(26.) Après ces essais sur des reptiles, sur des oiseaux, et sur de petits quadrupedes, je procedai à d'autres animaux plus grands, lapins, chiens, agneaux, bœufs; et non seulement je parvins à produire de semblables effets par toutes les manières décrites, mais à en obtenir de plus marqués et plus durables, à raison que la chaleur vitale se soutenoit dans ces grands animaux, et dans leurs membres, plus long-tems. Car je ne dois pas négliger de dire, que si dans la plûpart des animaux à sang froid, et particulièrement dans les grenouilles, la vitalité subsiste dans les membres tronqués plusieurs heures, cette vitalité qui les rend si sensibles à la plus foible irritation électrique, elle ne dure gueres que quelques minutes dans les membres découpés des animaux à sang chaud, et disparoit communément avant que toute cette chaleur animale soit dissipée.

(27.) Ayant eu un tel succès de mes expériences sur des animaux grands et petits de toute espèce, tantôt vivants et dans toute leur intégrité, tantôt écorchés, quelquefois decapités, et dissequés de différentes manières, et dans chacun de leurs gros membres tronqués, et presque toujours sans cette préparation requise par Mr. GALVANI, c'est à dire, sans mettre à decouvert les nerfs, je voulus aller plus loin, et essayer sur de petits membres, sur un seul muscle, et sur de petits morceaux de muscles ; et le nouveau succès que j'en ai eu m'a conduit à d'autres découvertes, que j'exposerai bientôt, après avoir décrit quelques unes de ces expériences.

(28.) EXPERIENCE E. J'ai coupé tantôt une jambe avec la cuisse, tantôt la jambe seule, tantôt une moitié ou un quart de jambe, à une grenouille ; et ayant appliqué, à l'ordinaire, à une partie de la piece coupée la feuille d'étain, et à une autre partie la lame d'argent, et fait communiquer entr'elles ces armures, j'obtins toujours des convulsions et mouvements. J'en ai détaché un seul muscle, par exemple le *gluteus* ou le *gastrocnemius* ; d'autres fois je n'en ai pris qu'un morceau pas plus gros qu'un grain d'orge ; mêmes effets, savoir, des contractions très vives et spasmodiques de ces muscles, ou de ces morceaux de muscles, moyennant l'artifice de deux armures differentes, &c.

EXPERIENCE F. J'ai répété les mêmes expériences sur une jambe, une moitié et un tiers de jambe, sur un seul muscle, et un fragment de muscle, de poulet, et d'autres oiseaux ; sur une tranche du *gluteus* d'un lapin, d'un agneau, &c. et j'ai eu les memes effets tout le tems que les chairs ont conservé une chaleur sensible. (sect. 26.)

(29.) Ainsi donc on excite des contractions très fortes dans les muscles des animaux à sang chaud, comme à sang froid, et dans toutes les parties coupées des muscles; et on les excite par le simple artifice des armures métalliques différentes, appliquées au muscle lui-même, sans aucune préparation des nerfs, même sans decouvrir ceux-ci. Ailleurs nous avons vu qu'on les excite également, et par le même moyen des armures appliquées à deux parties voisines du nerf seul, (sect. 19. et 20. Expérience A. et B.) d'où j'ai raison de conclure qu'il n'est pas du tout necessaire qu'il se fasse une décharge de fluide électrique entre nerf et muscle, ou qu'il s'en transporte de l'interieur à l'exterieur de ce dernier par le nerf et par l'arc conducteur, comme Mr. GALVANI suppose, ou *vice versa*; et qu'il n'y à aucune comparaison à faire du muscle avec la bouteille de Leyde et sa décharge, dans les expériences dont il s'agit ici. Qu'y a-t-il en effet qui resemble, et qu'on puisse expliquer analogiquement à la bouteille, lorsque les deux lames de métal, auxquelles arrivent les deux bouts de l'arc conducteur, se trouvent appliquées très près l'une de l'autre à l'extérieur du même nerf, (Expérience A. et B.) ou sur l'extérieur de deux muscles semblables, ou sur le même muscle; (Expérience C. D. E. F.) il faut convenir qu'on feroit inutilement des efforts pour soutenir ici une analogie avec la bouteille de Leyde.

(30.) EXPERIENCE G. Ayant revêtu de deux feuilles, une d'argent l'autre d'étain, les deux cuisses d'une grenouille aux endroits precisément correspondants, on excite les contractions des muscles, et les mouvements ordinaires des jambes, au moment qu'on fait communiquer par un arc conducteur ces deux armures.

(31.) Est ce comme cela, je demande, que se fait la décharge de deux bouteilles de Leyde, en établissant une communication entre les surfaces homologues? Laissons donc là ces idées de bouteille et décharge, et toute explication forcée, et disons simplement qu'il se fait ici, et dans les expériences analogues, un transport de fluide électrique de l'une à l'autre des deux parties convenablement armées; transport déterminé, non par un *exces respectif* de ce fluide, qu'on ne sauroit naturellement supposer entre des parties similaires, mais par la diversité de ces mêmes armures, qui doivent être de différents métaux, comme j'ai eu soin d'indiquer deja, (sect. 20. et 21. Expérience B. et C.) et toujours inculqué dans la suite. En effet,

(32.) EXPERIENCE H. Si deux muscles, ou deux endroits d'un seul muscle, sont armés pareillement, c'est à dire, de deux lames d'un même métal, égales aussi quant à leur trempe et dureté, souplesse ou rigidité, quant au poli ou à la rudesse des superficies, et appliquées de la même manière, on aura beau les faire communiquer par un arc conducteur, il ne s'ensuivra aucune convulsion, aucun mouvement.

(33.) J'avoue qu'il n'est pas aisé de concevoir comment et pourquoi la simple application de deux armures dissemblables, je veux dire de deux différents métaux, à deux parties similaires de l'animal, et même à des points très proches les uns des autres d'un muscle quelconque, trouble l'équilibre du fluide électrique, et, le tirant de son repos et de son inaction, le sollicite de passer incessamment d'un endroit à l'autre; lequel transflux a lieu sitôt qu'on établit un arc conducteur entre ces deux armures dissemblables, et continue tout le tems que cette communication subsiste. Mais concevable ou non, qu'en soit la cause, c'est un fait que les expériences deja rapportées prouvent assez, et qui sera confirmé par beaucoup d'autres; à la suite desquelles je tâcherai d'en donner quelqu'explication. C'est un fait qu'on doit ajouter à ce que nous connoissions deja en électricité; un fait qui doit surement paroître extraordinaire, et difficile à concilier avec les lois communément établies. C'est véritablement une nouvelle loi bien singuliere, que j'ai découverte; une loi qui n'appartient pas proprement à l'électricité animale, mais à l'électricité commune, puisque ce transflux de fluide électrique, transflux qui n'est pas au surplus momentané, comme seroit une décharge, mais continu et suivi tout le tems que la communication entre les deux armures subsiste, a lieu, soit que celles-ci se trouvent appliquées aux substances animales vivantes ou mortes, ou à d'autres conducteurs non métalliques, mais suffisamment bons, comme à l'eau, ou à des corps mouillés. Mais avant que d'en venir aux expériences qui prouvent décidément tout ce que j'avance ici, je dois encore m'arrêter quelque peu sur celles que j'ai deja rapportées. (sect. 20.—32.)

(34.) Il paroit d'abord par celles-ci qu'on peut exciter, moyennant le simple artifice des armures de différents métaux convenablement appliquées, de fortes contractions dans tous les muscles de tous les animaux, tant qu'ils jouissent encore de quelque vitalité. Une telle conclusion seroit pourtant trop génerale, et l'expérience même, au milieu des preuves que j'ai si fort étendues, m'a appris qu'il faut y mettre des restrictions, tant relativement aux classes et genres d'animaux, que par rapport aux différents muscles de chaque animal.

(35.) Et premièrement pour ce qui est des différentes classes d'animaux ; quoiqu'il soit bien constant que tous les quadrupedes, les oiseaux, les poissons, les reptiles, et les amphibies, que j'ai soumis aux épreuves, presentent les phénomenes décrits, il n'en est pas moins vrai que les vers en géneral, et plusieurs insectes, s'y sont refusés. J'ai essayé en vain les vers de terre, les sangsues, les limaces et limaçons, les huitres, et diverses chenilles ; je n'y ai pas même pu exciter des mouvements par de petites et médiocres étincelles, et décharges, d'électricité artificielle. Voici de quelle manière j'ai procédé.

EXPERIENCE I. J'ai appliqué la feuille d'étain, et la lame d'argent, à différentes parties, tant exterieures qu'interieures, de ces limaces, sangsues, vers de terre, &c. et le mieux qu'il m'a été possible ; et j'ai établi la communication de ces armures métalliques, tantôt en approchant l'une de l'autre jusqu'au contact, tantôt par l'intermede d'un autre métal faisant office d'arc conducteur ; mais par tous ces moyens je n'ai jamais pu obtenir le moindre mouvement dans aucune partie de leur corps.

EXPERIENCE L. J'ai affectué à travers leurs corps, isolés ou non isolés, des décharges de bouteille assez fortes pour exciter une médiocre étincelle, et pour me donner une petite commotion, et ils n'en furent pas sensiblement affectés ; point de mouvements ou des convulsions.

(36.) Est-ce donc que les animaux les plus imparfaits, la classe entière des vers, et plusieurs insectes ne possederoient gueres cette sensibilité et irritabilité, cette mobilité électrique, s'il m'est permis de dire ainsi, dont jouissent les autres animaux plus parfaits ? Je ne veux point encore tirer cette conclusion génerale de mes expériences, que je n'ai étendues jusqu'à présent qu'à un petit nombre de vers et d'insectes. Encore, à l'égard de ces derniers, je dois dire que j'ai réussi, sans beaucoup de difficulté, sur des ecrevisses, des scarabés, des sauterelles, des papillons, des mouches. Il ne sera pas inutile que j'explique une des manières par lesquelles je viens à bout avec ces animaux, difficiles d'assujettir aux expériences, ou par leur petitesse, ou par les écailles dont ils sont recouverts.

Experience M. Après avoir tranché la tête à la mouche, au papillon, au scarabé, &c. je leur fend, tout au long, le corcelet avec un canif, ou de petits ciseaux ; et j'introduis profondément dans la fente, près du cou, un morceau de feuille d'étain, (le papier dit improprement argenté est très à propos) et un peu au dessous j'introduis, de même bien avant dans l'intérieur, le tranchant d'une lame d'argent, ou d'une petite monnoye : alors quand j'avance celle-ci jusqu'au contact de la feuille d'étain, les jambes commencent à se plier, à se debattre, et les autres parties, et le tronc même, à s'agiter. Il est fort amusant d'exciter de cette manière le chant d'une cigale, &c.

(37.) Ainsi donc j'aurois grand tort de ranger les insectes parmi les animaux destitués, comme le sont les vers deja indiqués, de la faculté électrique dont il s'agit. Tout au plus, si les chenilles se montrent telles, on peut dire que dans cet état de larve, avant d'atteindre par leur métamorphose l'état parfait, d'acquerir de nouveaux organes, &c. de même qu'elles sont comparables aux vers à plusieurs autres égards, elles le sont aussi à celui de n'être pas douées de la sensibilité électrique.

(38.) Enfin, s'il m'est permis de dire ici ce que je pense, les animaux seulement qui ont des membres bien distincts, des articulations, et des muscles propres pour le mouvement de chacun, de ces muscles qu'on appelle flexeurs, ou élevateurs, et des nerfs propres qui les regissent, se ressentent, et sont saisis d'une contraction reélle et spasmodique, soit par de petites décharges d'électricité artificielle, soit par un foible courant de fluide occasionné par les simples armures métalliques differentes; contractions et spasmes qui entrainent le mouvement, et aussi l'agitation violente des dits membres. Au contraire les vers, et ceux d'entre les insectes qui n'ont point de membres assez distincts, point d'articulations proprement dites, ou qui manquent de ces muscles flexeurs, ou qui ne jouissent que d'un mouvement vermiculair, ne sont point affectés par une semblable électricité. C'est une tout autre économie animale, une tout autre méchanique pour les mouvements de ces animaux, un jeu qu'on a très bien découvert et expliqué dans plusieurs espèces. Voila mes idées, encore un peu vagues, fondées sur quelques expériences ; c'est la suite de celles-ci qui doit ou les confirmer, ou les rectifier.

(39.) A l'égard des différents muscles dans le même animal, je suis en état d'avancer quelque chose de plus assuré. Je dis donc, qu'il s'en faut de beaucoup que tous les muscles soient susceptibles de contraction par la foible action électrique dont il s'agit. Il y a une grande distinction à faire par rapport à leur fonction dans l'économie animale; tous ne sont pas soumis à l'empire de la volonté, et prêts aux mouvements spontanés. Or, il n'y a proprement que ceux-ci qui soient capables des contractions spasmodiques, par les moyens décrits. Oui, il n'y a que les muscles obeissants à la volonté que j'ai trouvés susceptibles d'irritation et de mouvement, par l'action de ce foible courant de fluide électrique occasionné par le simple attouchement de deux métaux différents; et point du tout les autres muscles sur lesquels la volonté n'a aucun pouvoir direct, comme ceux du ventricule, des intestins, &c. pas même le cœur, d'ailleurs si irritable. Les muscles du diaphragme oui; (et je le devinai avant que d'en faire l'épreuve) puisqu'ils sont d'entre ceux dont les mouvements dépendent de la volonté.

Experience N. Il est bien surprenant qu'une tranche de bonne chair musculaire, coupée, par exemple, à la cuisse d'un agneau égorgé une demie heure ou une heure avant; que ce morceau, dis-je, de muscle presqu'entièrement refroidi, et qui ne se ressent plus de l'action d'aucun stimulant méchanique ou chymique, soit si puissamment affecté par le fluide électrique transmi d'une partie à l'autre, au point d'être saisi de contractions spasmodiques très fortes; et qu'au contraire le cœur recemment arraché à ce même animal, et encore tout chaud et très irritable, traité de même, sollicité également par des armures métalliques le mieux adaptées, et l'arc conducteur qui en établit la communication, n'en souffre aucune altération; que ses battements lorsqu'ils sont affoiblis et lents ne redoublent point, et lorsqu'ils sont suspendus ou assoupis ne se reveillent pas, tandis que cela arrive par les plus foibles stimulants méchaniques, ou chymiques.

(40.) Le fluide électrique donc, qui paroît être le stimulant approprié aux muscles de la volonté, ne l'est aucunement pour le cœur, et pour les autres muscles doués des mouvements vitaux et animaux non volontaires. Mais que dira-t-on si je montrerois qu'il n'est pas non plus la cause immédiate, ou efficiente, des mouvements des dits muscles volontaires; que dans ceux-ci mêmes il n'est encore qu'une cause médiate, entant que les nerfs seuls en sont directement affectés? C'est ce que plusieurs expériences m'ont appris; par lesquelles j'ai été forcé de renoncer aux plus belles et vastes idées. J'aimois à penser, avec Mr. GALVANI, que le fluide électrique mis en mouvement dans les organes, toutes les fois qu'il poussoit son courant jusqu'aux muscles, et qu'il les frappoit avec une certaine force, fit lui-même l'office de stimulant, et excitât l'irritabilité qui leur est propre; que tous les mouvements musculaires s'executassent par une semblable irruption de fluide électrique dans les muscles, soit lorsqu'on employoit l'électricité artificielle, soit lorsqu'on donnoit jeu à l'électricité animale naturelle; qu'enfin les mouvements mêmes qui se font naturellement dans la machine animale vivante, au moins les mouvements volontaires, reconnussent la même cause, savoir, l'action immédiate du fluide électrique sur les muscles. Mais, je le repéte, j'ai du renoncer, non sans regret, à toutes ces belles idées, par lesquelles il nous paroissoit possible d'expliquer les choses à merveille. Oui, il faut limiter beaucoup l'action de l'électricité dans les animaux, et l'envisager sous un autre point de vue, savoir, comme capable seulement d'exciter par elle-même les nerfs, comme j'ai déja indiqué, et comme je vais maintenant prouver.

(41.) D'abord qu'elle puisse agir, et qu'elle agisse effectivement, sur les nerfs, et que ceux-ci excités par elle excitent à leur tour les muscles dépendants, sans même que le courant électrique arrive jusqu'aux dits muscles, c'est un fait qui n'a plus besoin de preuves après celles fournies par les Expériences A. et B. (sect. 19. et 20.) et même par une expérience de Mr. GALVANI, qui fut la première de toutes, et l'origine des autres, suivant son recit. On voit assez que le courant électrique, dans cette expérience du professeur de Bologne, comme dans les miennes que je viens de citer, traverse une partie seulement du nerf crural, et pas un des muscles de la jambe; cependant comme ils dépendent de ce nerf, ils tombent tous en convulsion.

(42.) Mais je vais plus avant et je soutiens, que même dans les cas où le courant électrique (on comprend bien que je n'entends parler que des foibles décharges artificielles, ou de ce courant qui a lieu par la simple application des armures de différents métaux) frappe et pénetre les muscles susceptibles de mouvement, ce n'est pas en irritant ceux-ci immédiatement qu'il les fait entrer en contraction, mais en stimulant leurs nerfs. C'est ce qu'indiquent déja mes Expériences C. et D. (sect. 21. et 23.) où la feuille d'étain et la lame d'argent se trouvant appliquées immédiatement aux parties musculeuses de l'animal, soit entier, soit écartelé, ce ne sont pas tant les muscles couverts par les deux armures métalliques qui souffrent les plus violentes contractions, que ceux qui dépendent de quelque nerf principal, auquel soit proche l'une ou l'autre des armures. C'est ainsi que dans la grenouille, lorsque la feuille d'étain est appliquée sur les reins, ou gissent à peu de profondeur les nerfs cruraux, les muscles des jambes sont saisis de fortes convulsions plus que tout autre, plus même que ceux qui touchent ou avoisinent l'autre armure, c'est à dire, la lame d'argent. J'ai déja fait observer la même chose dans les quadrupedes, chiens, agneaux, &c. par rapport au nerf ischiatique, (Expérience D.) et je dois ajouter seulement, que la jambe ne laisse pas d'être sécouée lorsque ce nerf n'est pas trop caché sous les chairs et autres intéguments, et on applique comme il faut à cet endroit une des armures ; quand même on ne feroit point repondre l'autre ni au muscle *gluteus,* ni à aucun muscle de la jambe, mais à un autre quelconque, pourvu qu'il ne soit pas trop éloigné. Voila encore pourquoi,

EXPERIENCE O. Si on applique à la grenouille, ou à d'autres petits animaux, la feuille d'étain tout le long de l'épine du dos, d'où sortent tous les nerfs du tronc et des membres; et l'autre armure à une autre partie quelconque, tous ces membres se debattent, les muscles, non seulement des jambes mais du ventre et du dos, souffrent des contractions spasmodiques, et le tronc lui-même se courbe et se plie en arc ; en un mot les convulsions sont génerales. L'expérience est encore plus frappante dans un lezard que dans une grenouille, et je vais la décrire.

EXPERIENCE P. Ayant coupé la tête à un lezard, et découvert les muscles du dos en enlevant la peau, j'applique un morceau de feuille d'étain au bout tronqué, de manière que cette feuille deborde un peu et s'éleve sur les épaules, et je pose une monnoye d'argent sur le milieu de l'épine ; enfin je fais avancer, en glissant, cette monnoye jusqu'au contact de la dite feuille. A l'instant les jambes remuent, la queue se replie tortueusement, et tout le corps agité se courbe et s'élance de droite à gauche, et de gauche à droite. N'est-ce pas à cause que la partie supérieure de la moelle épiniere, la source principale des nerfs, est irritée ?

(43.) On peut obtenir, par une semblable opération, à-peu-près les mêmes effets dans une souris, un petit oiseau, &c.

mais il faut en ôter, non seulement la première peau et les autres intéguments, mais aussi de la chair, à raison que leur dos est plus charnu, et les principaux nerfs et la moëlle se trouvent plus cachés par cette chair, et par les os mêmes du tube vertebral. Il est aisé en effet de comprendre que le courant de fluide électrique, occasionné par les deux armures, ne penetrant qu'à une certaine profondeur les parties de l'animal recouvertes par ces armures, ne peut guere atteindre ni la moëlle épiniere, ni les principales branches des nerfs, qui entrent dans l'intérieur des membres, si les os, la chair, et d'autres intéguments interposés ont une epaisseur considerable. On comprend aussi pourquoi dans les grands animaux, chiens, agneaux, &c. on ne réussit pas à exciter de cette manière des mouvements dans tous les membres, je veux dire en appliquant les deux armures au dos, quoique décharné. Les gros troncs des nerfs restent encore trop cachés, et ensevelis; il n'y a que des branches ou ramifications qui gissent peu au dessous des dites armures, et ces branches n'aboutissent, pour la plus-part, qu'à certaines parties extérieures et voisines; en conséquence on ne voit naitre communément que des contractions et des palpitations superficielles dans tel ou tel autre muscle. Ou si par hazard tout un membre est mis en mouvement, c'est que le nerf qui entre dans son intérieur, et regit ce mouvement, se trouve peu caché, qu'il n'y a qu'un leger voile, une couche peu epaisse qui le couvre, des fibres minces seulement interposées entre lui et l'une ou l'autre des armures métalliques; comme on a pu observer dans les Expériences D. et suivantes, (sect. 23, &c.) où il suffisoit, pour exciter de grands mouvements dans la jambe d'un chien, ou d'un agneau, d'appliquer une des armures près du nerf ischiatique, et plus on en approchoit, et plus on amincissoit la couche de chair qui l'enveloppoit, plus les mouvements de la jambe étoient forts.

(44.) Il faut donc connoître la position des nerfs, leur direction, &c. et il faut enlever non seulement les intéguments communs, la graisse, &c. mais aussi partie de la chair qui couvre et enveloppe les dits nerfs, il faut amincir plus ou moins cette enveloppe, avant que d'y appiiquer l'armure métallique, pour obtenir dans les grands animaux le mouvement de tel ou tel autre membre, outre les contractions et palpitations superficielles de quelques muscles. Il est peut-être impossible d'exciter ces mêmes mouvements et convulsions dans tous les membres à la fois ; tandis que cela n'est pas difficile dans les petits animaux, comme nous avons vu ci-dessus, (sect. 42. Expérience O. et P.) en leur ôtant seulement la peau, ou partie des autres intéguments ; ce qui n'est pas meme nécessaire pour la grenouille, à laquelle on peut laisser la peau, qui, étant extremement mince et humide, n'empéche pas par son interposition que le courant électrique atteigne les principaux nerfs, ou la moëlle épiniere.

(45.) Mais s'il faut avoir égard à la direction des principaux nerfs, pour determiner les mouvements dans les différents membres, il faut aussi faire attention à la position des armures relativement aux muscles; puisque ceux qui se trouvent interposés, et plus près de l'une ou de l'autre armure, sont en géneral plus sujets à contracter des convulsions spasmodiques, et souvent aussi sont les seuls dans lesquels on les observe ; par exemple, lorsque les armures ne repondent à aucun gros nerf, ou, s'il y en a, lorsqu'ils se trouvent trop enveloppés et trop profondément cachés.

(46.) Cela, et les Expériences E. F. (sect. 28.) où un muscle seul, et même un morceau de muscle, traité à l'ordinaire, ne laisse pas de souffrir des contractions très fortes, pourroient faire croire que le fluide électrique produisît ces mouvements en irritant les fibres musculaires elles-mémes, sans l'intervention des nerfs ; l'action desquels par conséquent ne seroit ni *primaire*, ni absolument nécessaire, comme je pretends. Mais l'argument tiré de ces exemples n'a aucune force, tant qu'on ne prouve pas que dans ces muscles, dans ces morceaux de muscle, il n'y ait gueres de nerfs ; puisque s'il y en a, (et certainement il doit y avoir, et il y a, des ramifications nerveuses dans chaque portion sensible, j'ai presque dit dans chaque fibre musculaire) je puis toujours soutenir que ce sont ces filets nerveux, dont la substance du muscle se trouve parsemée, qui sont immédiatement affectés par le fluide électrique qui pénetre cette même substance ; que ce fluide déployant son action sur les nerfs extremement sensibles, action qui finit là, ceux-ci exercent la leur sur les muscles, &c. Je puis, dis-je, soutenir avec assez de vraisemblance que le fluide électrique n'a par lui-même d'influence au phénomene des contractions musculaires, qu'en ce qu'il en excite les nerfs ; en un mot, qu'il n'en est pas la cause immédiate. Une telle assertion, que les choses expliquées jusqu'ici rendent plus que probable, est prouvée directement, et de la manière la plus évidente, comme je vais montrer, par plusieurs expériences que j'ai faites sur la langue : expériences qui m'ont conduit à d'autres découvertes, aussi intéressantes que curieuses.

(47.) Etant parvenu à exciter des convulsions toniques, et les mouvements les plus forts, dans les muscles, et dans les membres, non seulement des petits mais des grands animaux, sans découvrir aucun nerf, par la simple application des armures de différents métaux aux muscles denués des intéguments, je pensai bien-tôt si on ne pourroit pas obtenir la même chose dans l'homme. Je conçus que la chose réussiroit très bien dans les membres amputés ; mais dans l'homme entier et vivant comment faire? Il auroit fallu aussi ôter les intéguments, faire des incisions profondes, emporter même une partie des chairs aux endroits sur lesquels on alloit appliquer les lames métalliques, (comme j'ai fait remarquer qu'il faut faire souvent aux parties charnues des grands animaux). Heureusement il me vint dans la tête, que nous avons, dans la langue, un muscle nu, dépourvu au moins des intéguments épais dont sont couvertes les parties extérieures du corps, un muscle qui est très mobile, et mobile à volonté. Voila donc, me disois-je, toutes les conditions requises, pour pouvoir y exciter de vifs mouvements par l'artifice ordinaire des armures différentes. Dans cette vue je fis, sur ma propre langue, l'expérience suivante.

(48.) Experience Q. Ayant revêtu la pointe de la langue, et une partie de sa surface superieure, dans l'étendue de quelques lignes, d'une feuille d'étain, (le papier dit argenté est le plus à propos) j'appliquai la partie convexe d'une cuiller d'argent plus avant sur le plat de la langue, et en inclinant cette cuiller je portai sa queue jusqu'au contact de la feuille d'étain. Je m'attendois à voir tremblotter la langue ; et je faisois, pour cela, l'expérience devant un miroir. Mais les mouvements que j'osois prédire n'arriverent pas ; et j'eus, au lieu de cela, une sensation à laquelle je ne m'attendois nullement ; ce fut un goût aigre assez fort, sur la pointe de la langue.

(49.) Je fus d'abord fort surpris de cela; mais reflechissant un peu à la chose, je conçus aisément, que les nerfs qui aboutissent à la pointe de la langue, étant les nerfs destinés aux sensations du goût, et nullement aux mouvements de ce muscle flexible, il étoit tout-à-fait naturel, que l'irritation du fluide électrique, mu par l'artifice ordinaire, y excitât une saveur, et pas autre chose; et que pour exciter dans la langue les mouvements dont elle est susceptible, il faudroit appliquer une des armures métalliques auprès de sa racine, où s'implantent les nerfs destinés à ces mouvements; ce que je verifiai bientôt par cette autre expérience.

(50.) Experience R. Ayant coupé, à un agneau tout recemment égorgé, la langue près de sa racine, j'appliquai une feuille d'étain à l'endroit de la coupure, et la cuiller d'argent à une de ses surfaces; procedant alors à établir une communication, comme il faut, entre ces deux armures métalliques, j'eus le plaisir de voir la langue entière tremousser vivement, élever sa pointe, se tourner et se replier de part et d'autre, chaque fois et tout le tems qu'une telle communication avoit lieu.

(51.) J'ai répété cette expérience sur une langue de veau, que je posai, armée de la même manière de la feuille d'étain près de sa racine, sur un plat d'argent, pour qu'il fît l'office de l'autre armure; et le succès fut le même. Je l'ai répétée aussi sur la langue d'autres petits animaux, comme souris, poulets, lapins, &c. et j'obtins presque toujours le même effet. Je dis presque toujours, car quelques fois il manqua dans la langue des petits animaux; soit que la feuille d'étain ne fût pas appliquée convenablement à l'endroit juste, où les nerfs qui regissent les mouvements de la langue y ont leur insertion; soit que la langue refroidie eût déja perdu sa vitalité, qui ne dure gueres long-tems dans les muscles des animaux à sang chaud, comme j'ai déja fait observer, (sect. 26.) et particulièrement dans la langue.

Je suis, &c.

A. VOLTA.

Octobre 25, 1792.

V. *Further Particulars respecting the Observatory at* Benares, *of which an Account, with Plates, is given by Sir* Robert Barker, *in the LXVIIth Vol. of the Philosophical Transactions. In a Letter to* William Marsden, *Esq. F. R. S. from* John Lloyd Williams, *Esq. of* Benares.

Read January 31, 1793.

DEAR SIR,

In conformity with your request, I have now the pleasure of sending you an account of the measurement of the different parts of the Benares observatory, called *maun-mundel*, as taken by myself, with a two-foot rule, and a rod of ten feet very exactly divided. An account of the use of the different instruments, though very imperfect, was given me on the spot, by several learned Brahmins who attended me; one of whom is professor of astronomy in the new founded college at Benares. They all agreed that this observatory never was used, nor did they think it capable of being used, for any nice observations; and believe that it was built more for ostentation, than the promotion of useful knowledge.

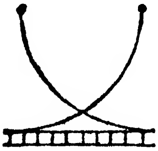
In my inquiry into the particulars of the building, I have been assisted by my friend the Nabob Ali Ibrahim Kaun, and I believe this account may be relied on.

A.* The large quadrant, called in Arabic, *kootoop-bede*; in Hindoo, *droop*, the name of the north polar star. This instrument is built of stone, fixed in mortar, and clamped with iron in a very clumsy manner; between most of the stones are spaces of $\frac{1}{16}$ part of an inch. The stile, in its length from north to south, measured 39 feet $6\frac{1}{2}$ inches; the height of the south end, 5 feet $4\frac{1}{4}$ inches; height of the north end, 22 feet 3 inches. This stile consists of two walls $11\frac{1}{2}$ inches thick, with a flight of 27 steps between; and on the outer edge of each of these walls are fixed two iron rings. The distance between the two rings is 5 feet $8\frac{1}{2}$ inches; from the uppermost to the top, 18 feet 8 inches; from the lower one to the bottom, 15 feet and $\frac{1}{2}$ an inch; both sides are nearly alike. The rings are, each of them, $\frac{3}{4}$ of an inch in thickness, and they are let into the wall between two stones; the holes through which the object is to be viewed are $\frac{5}{16}$ths of an inch in diameter, $\frac{3}{8}$ths of which space, in each, is covered by the projection of the stone. The radius of one of the quadrants, on which the hour lines are marked, from the outer part of the wall of the stile to the inner edge of the arc, is 9 feet and $\frac{3}{4}$ths of an inch; that of the other, 9 feet one inch. The width of the rim of the quadrants, which are inclined to a line perpendicular to the shadow falling from the gnomon, is 5 feet $10\frac{1}{4}$ inches. The quadrant is divided into 6 *gurries*, and each *gurry* into 10 *pulls*.

On the outer wall of the stile, fronting the east, at the

* The references are to the plates annexed to Sir ROBERT BARKER's account.

height of 10 feet and 10 inches from the base, are fixed two iron pins, each forming a centre, from which circular lines are drawn, intersecting each other, as in the annexed representation;



with a parallel line drawn underneath, which has the hour, or *gurry* and *pull* lines marked on it. The wall is plastered; and there are, on other edifices fronting the east, similar lines drawn; the use of which, I understood, was to ascertain the time of the day.

B. An equinoctial dial, called *gentu-raje.*—It is a circular stone, fronting north and south, but inclining towards the south. The diameter of the south face is 2 feet $2\frac{3}{4}$ inches, a perpendicular line falling from the top will give one foot distance from the bottom of the inclined plane. In the south front of this stands a small stone pillar, distance 3 feet 8 inches; a line drawn from the centre of this dial to the point on the top of the pillar, will, by its shadow, give the time of the day. On the *nadir* side of this dial, the stone is 4 feet 7 inches diameter: on the centre of which is a small iron stile, with a hole in it, perpendicular to its plane; and in the perpendicular line of the chord are placed two small irons. A line passing through the hole in the stile, and each end applied to the forementioned irons, gives a shadow, which denotes the hour, &c.

C. A brass circle in the line of the equator, facing north and south. It has a moveable index, turning on a pivot in the centre; the circle is divided into 360 degrees, or *unse*, subdivided again into 60′, and again into 6″, and into $\frac{1}{4}$ths. This instrument is called *cund-brit*, or *cranti-brit*, but I could not learn the use of it.

D. A double circular wall, with a round pillar in the centre, as described by Sir ROBERT BARKER. The floor being broken, and uneven, renders the height of the outer wall irregular, but it measured from 8 feet 1 inch, to 8 feet 3 inches; diameter inside, 27 feet $6\frac{1}{2}$ inches; thickness of the wall, 2 feet. The inner wall is 18 feet within; thickness of this wall, 1 foot $5\frac{1}{2}$ inches. The diameter of the centre pillar, 3 feet $7\frac{1}{2}$ inches.

At the four cardinal points, on the top of the outer wall, are four iron pins, with small holes in them, through which, the Pundits say, wires are designed to be drawn at the time of observation, which wires intersect each other at the centre of the pillar. The tops of both the walls are graduated, or divided into degrees; and it is said, that by the shadow of these wires falling on the walls, the sun's declination is found.

In addition to the foregoing, which are described in the plates alluded to, on the south-east quarter of the building is a large black stone, 6 feet 2 inches diameter, fronting the west; it stands on an inclined plane. I could not learn the use of this instrument; but was informed that it never had been completed. There is no other building of any consequence, nor does it appear there ever was.

I fear, that from the want of sufficient knowledge of the science of astronomy, I have not been able to describe the different instruments, and their uses, satisfactorily; however, you may rely on the measurements being taken with the greatest exactness.

For the following description I am indebted to our chief magistrate, the Nabob ALI IBRAHIM KAUN.

" The area, or space comprising the whole of the buildings and instruments, is called in Hindoo, *maun-mundel*; the cells, and all the lower part of the area, were built many years ago, of which there remains no chronological account, by the Rajah MAUNSING, for the repose of holy men, and pilgrims, who come to perform their ablutions in the Ganges, on the banks of which the building stands.

" On the top of this the observatory was built, by the Rajah JEYSING, for observing the stars, and other heavenly bodies; it was begun in 1794 Sumbut, and, it is said, was finished in two years. The Rajah died in 1800 Sumbut.

" The design was drawn by JAGGERNAUT, and executed under the direction of SADASHU MAHAJIN; but the head workman was MAHON, the son of MAHON a pot-maker of Jeypoor. The pundit's pay was five rupees per day; the workmen's two rupees, besides presents; some got lands, or villages, worth 3 or 400 rupees yearly value; others money."

I am, &c.

Benares
March 25, 1792.

J. LL. WILLIAMS.

VI. *Extracts of two Letters from the Rev.* **Edward Gregory**, *M. A. Rector of* Langar, Nottinghamshire, *to the Rev.* Nevil Maskelyne, *D. D. F. R. S. Astronomer Royal*; *containing an Account of the Discovery of a Comet, with Observations thereon.*

Read February 7, 1793.

Extract of the first Letter, dated Langar, *near* Nottingham, January 10th, 1793.

I TROUBLE you with this letter to communicate to you such observations as I have made of a comet, which I saw on the 8th instant. The evening of the 8th being very clear, I was employed in my observatory, in taking differences of right ascension and declination between the planet Venus and ι Aquarii, when, happening to look towards the north-west part of the hemisphere, I saw a star of a hazy appearance, and about the size of a star of the second magnitude, in the space between the flexure of the Dragon and the foot of Hercules, larger and brighter than I had before remarked in that part of the heavens; which excited my attention so much, as to induce me to direct such a telescope to it as lets in much light, and is generally used at sea to see objects in the night. This star seemed to have a hazy and indistinct appearance in the telescope, which immediately led me to suspect it might be a comet; but the twilight yet remaining, I was not quite certain of it. When the night was completely come on, it became evident it was a comet, the coma being of a white light, hazy, and ill defined. I could perceive no nucleus, nor as yet any appearance of a tail.

I waited for, and was fortunate enough to obtain, an observation of its passage over the meridian under the pole, at $4^h\ 8'\ 30''$ by EARNSHAW's clock, or $4^h\ 6'\ 43''$ sidereal time; its zenith-distance, by BIRD's quadrant, being $75°\ 16'\ 16''$. The observation of the passage over the meridian was taken by guessing when a hazy dim appearance, about the shape and size of a hen's egg, was in the centre of the field of the transit instrument; any light, however weak, effacing all the light of the comet.

These observations were merely formed from the best judgment I could make by the naked eye, for, as I before observed, the light of the comet was so weak, as not to bear any degree of light sufficient to render visible the wires in the night telescope; which I have mounted on a polar axis, with a proper system of wires to take differences of right ascension and declination.

I continued to watch the comet until three o'clock in the morning, when it had ascended to some considerable altitude; I could perceive with the night telescope a very faint, but yet sufficiently evident tail, and that the comet had moved a few minutes to the west; that is, had increased its right ascension, and also its polar distance. I then observed the comet with other telescopes, of less aperture and deeper powers; in such it appeared a confused white hazy light, nor could I perceive any nucleus or tail, although it was visible in the night-glass, with its direction towards the zenith.

In case you are already apprised of the appearance of this comet, and of course this information is superfluous, I hope you will attribute my giving you this trouble to my eager desire that these extraordinary bodies may be observed with such capital instruments as you are in possession of, and also to shew you, by these attentions, the sense I entertain of the many civilities I have received from you.

I am, &c.

EDWARD GREGORY.

On the 9th I obtained a very imperfect glimpse of the comet, the twilight yet being very considerable, and the air very hazy. The comet has moved considerably westward, and its polar distance, I think, is increased. I thought the tail appeared rather brighter and longer; the coma not altered; no nucleus to be seen. This observation was extremely imperfect, the comet being seen with the night-glass held in my hand, during a space of four or five minutes; the sky, in that part, was cloudless, though most of the surrounding constellations were obscured.

Extract of the second Letter, dated January *25th*, 1793.

The observations of the comet were taken after the following manner. Finding that any degree of light sufficient to render the wires visible effaced the comet, I brought it, as nearly as I could judge, into the centre of the field without using any light, and then cast light on the illuminator; and in that stage of the process between the comet's disappearing and the wires becoming visible, I trusted to the impression left on the eye for the place of the comet when it vanished; and

I think I could not err more than three or four minutes from its being on the middle horizontal wire, and about as much from the intersection of the vertical, with the middle horizontal wire. But, after all, it is a vain thing to talk of critical exactness in this matter, either in the quadrant or transit observations, under the circumstances I am going to describe to you. The comet, in the telescopes of these instruments, had somewhat the appearance of a hen's egg, seen obliquely, with the large end towards the eye; of a dull white misty light. I could perceive no nucleus, therefore I considered the longest diameter, which was nearly directed towards the east, (but there was hardly any perceivable difference in the diameters) as the line in which, had any nucleus been visible, it would have been found; and I endeavoured, in the manner I have described, to bring this line on the middle horizontal wire, and the brightest part of it up to the vertical wire; and when I thought I had effected this, I read off the divisions on the arch, and noted the time by EARNSHAW's regulator.

On January 21st, I observed the instantaneous immersion of γ Tauri, at $0^h\ 54'\ 22''$ by my clock; and at $2^h\ 3'\ 31''$ I saw it again about a minute's distance from the moon's enlightened limb, the moment of emersion having escaped observation. I had, on the same day, observed the sun's passage over the meridian, by a mean of the wires, at $20^h\ 18'\ 45\frac{1}{3}''$; the clock had been losing a little more than a second a day, for eight or ten weeks past.* The mean of ten observations of the first satellite

* Hence the immersion of the star was at $4^h\ 34'\ 48'',6$, and the first sight of it after the emersion at $5^h\ 43'\ 46''$, apparent time. This may perhaps be useful in determining the longitude of Mr. GREGORY's observatory.—Note by Dr. MASKELYNE.

of Jupiter place me 3′ 47″, in time, west of Greenwich. My latitude, deduced from a great number of observations of the sun and stars, in all the various ways of determining the latitude, with BIRD's quadrant, HADLEY's quadrant, and two equatorial instruments, is 52° 54′ 37″ N.

It remains that I relate what I saw of the comet's tail. At 15^h astronomical time of the 8th, or three o'clock civil time in the morning of the 9th, I saw a very faint beam of light extending itself from the coma towards the zenith. When I brought the coma to the centre of the field of the night-glass, which takes in about 7 degrees, it reached to near the circumference of the field, consequently it amounted to about $3\frac{1}{2}$ degrees. I thought it brighter and longer when I had a mere glimpse of the comet in the evening twilight on the 9th. On the 10th, 11th, and 12th, the tail was rather brighter, yet very faint; not broader than a finger, nor brighter than a beam of light let into an ill darkened room for prismatic experiments. It extended itself beyond the circumference, when the coma was in the centre of the night-glass, perhaps a degree, consequently was $4^\circ\frac{1}{2}$ long; it was inclined towards the east, and on the 12th pointed due east at midnight.

On the 11th, the comet passed the middle wire of the transit instrument under the pole, at 8^h 28′ 0″ sidereal time. The zenith distance was 56° 2′ 15″.

I am, &c.

EDWARD GREGORY.

VII. *Observations of the Comet of 1793, made by the Rev.* Nevil Maskelyne, *D. D. F. R. S. Astronomer Royal, and other Observers. Communicated by the Astronomer Royal.*

Read February 14, 1793.

1793.	Mean Time at Greenwich	Æ of Comet in sidereal Time.	Æ of Comet in Degrees, &c.	Declination of Comet.	Longitude of Comet.	Latitude of Comet.	Name of Observers.
Jan^ry.	H. M. S.	H. M. S.	S. ° ′ ″	D M. S.	S. D. M. S.	D. M. S.	
8	8 55 7	16 6 43	8 1 40 45	51 46 23 N	7 2 29 3	69 38 5 N	Mr. Gregory.
11	13 3 50	20 27 56	10 6 59 0	71 1 42 N	1 5 16 0	76 9 8 N	Mr. Gregory.
14	11 38 0	1 12 48	0 18 12 0	45 53 12 N	1 6 16 29	34 53 33 N	Mr. Steph. Lee.
18	9 26 6	1 58 28,6	0 29 37 9	17 47 7 N	1 3 45 36	5 19 33 N	The Astronomer Royal.
21	8 5 52	2 11 42,8	1 2 55 42	9 1 26 N	1 3 47 57	4 0 25 S	The Astronomer Royal.
26	6 54 35	2 22 42,23	1 5 40 33	1 48 19 N	1 3 58 51	11 43 25 S	The Astronomer Royal.
27	7 37 41	2 24 4.19	1 6 1 3	0 51 36 N	1 3 59 30	12 43 44 S	The Astronomer Royal.
28	6 56 35	2 25 18,35	1 6 19 35	0 4 11 N	1 4 1 22	13 34 36 S	The Astronomer Royal.
30	8 57 38	2 27 41,4	1 6 55 21	1 20 59 S	1 4 7 13	15 6 48 S	The Astronomer Royal.
Feb.							
3	7 11 52	2 31 27,2	1 7 51 48	3 17 27 S	1 4 22 44	17 15 20 S	The Astronomer Royal.
4	7 4 12	2 32 17,2	1 8 4 18	3 40 40 S	1 4 27 4	17 41 21 S	The Astronomer Royal.
5	7 42 28	2 33 5.13	1 8 16 17	4 2 42 S	1 4 31 19	18 6 6 S	The Astronomer Royal.
7	8 5 26	2 34 37.55	1 8 39 23	4 41 11 S	1 4 40 55	18 50 1 S	The Astronomer Royal.

These places of the comet may admit of some little change, when the stars with which it was compared have been settled by observations with the meridian instruments.

VIII. *Account of the Method of making Ice at* Benares. *In a Letter to* William Marsden, *Esq. F. R. S. from* John Lloyd Williams, *Esq. of* Benares.

Read February 14, 1793.

DEAR SIR,

As the method of making ice in this country, where the thermometer, during part of the year, stands at from 95 to 100° in the shade, has something peculiar in it, I trust the following description of the process will not be unacceptable.

You know that ice is made in India during the months of December, January, and part of February; but I believe it has generally been considered as necessary to the congelation of the water, that it should have been boiled. However, I can now assure you, as a fact within my own observation for these nine years past, that a large quantity of ice has been made at this place every year, without any preparation whatever; and I have often seen ice of an inch and quarter thick, notwithstanding I do not conceive that the atmosphere, at that time, was sufficiently cold to produce the effect; for I have frequently placed a thermometer, with the naked bulb on the straw, amidst the freezing vessels during the night, and on inspecting it between five and six o'clock in the morning (at which time the ice-makers informed me the cold was most intense), I never found it below 35°. I have even seen ice, of a considerable thickness, formed when the thermometer was not lower than 40 degrees.

The method of making ice at Seerore, near Benares, is as follows.

A space of ground of about four acres, nearly level, is divided into square plats, from four to five feet wide. The borders are raised, by earth taken from the surface of the plats, to about four inches; the cavities are filled up with dry straw, or sugar-cane haum, laid smooth, on which are placed as many broad shallow pans, of unglazed earth, as the spaces will hold. These pans are so extremely porous, that their outsides become moist the instant water is put into them; they are smeared with butter on the inside, to prevent the ice from adhering to them, and this it is necessary to repeat every three or four days; it would otherwise be impossible to remove the ice without either breaking the vessel, or spending more time in effecting it than could be afforded, where so much is to be done in so short a time. In the afternoon these pans are all filled with water, by persons who walk along the borders or ridges. About five in the morning, they begin to remove the ice from the pans; which is done by striking an iron hook into the centre of it, and by that means breaking it into several pieces. If the pans have been many days without smearing, and it happens that the whole of the water is frozen, it is almost impossible to extract the ice without breaking the pan. The number of pans exposed at one time, is computed at about 100,000, and there are employed, in filling them with water in the evenings, and taking out the ice in the mornings, about 300, men, women, and children; the water is taken from a well contiguous to the spot. New vessels, being most porous, answer best.

It is necessary that the straw be dry; when it becomes wet, as it frequently does by accident, it is removed, and replaced. I have observed water which had been boiled, freeze in a china plate; yet having frequently placed a china plate, with well-water, among the unglazed pans on the straw beds, I found that when the latter had a considerable thickness of ice on them, the china plate had none. I have also wetted the straw of some of the plats, and always found it prevented the formation of ice. The air is generally very still when much ice is formed; a gentle air usually prevails from the south-westward about daylight. I had a thermometer among the ice pans, during the season of making ice, with its bulb placed on the straw, and another hung on a pole $5\frac{1}{2}$ feet above the ground; and commonly observed, that when ice was formed, and the thermometer on the straw was from 37 to 42°, that on the pole would stand about 4 degrees higher; but if there was any wind, so as to prevent freezing, both the thermometers would agree.

I shall offer no opinion respecting the causes of ice being formed when the thermometer is so many degrees above the freezing point; but hope the subject will be elucidated by some more capable person.

I am, &c.

Benares,
March 25, 1792.

J. LL. WILLIAMS.

IX. *Account of two Instances of uncommon Formation, in the Viscera of the Human Body. By Mr.* John Abernethy, *Assistant Surgeon to St.* Bartholomew's *Hospital. Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read February 14, 1793.

I TAKE the liberty of presenting to the Royal Society, the relation of two cases of uncommon formation of the human body. When animal existence is supported by any other than the usual admirably contrived means, it cannot fail to excite the attention of the philosopher, since it shews to him the powers and resources of nature.

The peculiarities of the first case which I have the honour to offer to the Society, consist in an uncommon transposition of the heart, and distribution of the blood vessels; together with a very strange, and, I believe, singular formation of the liver. The body which contained these deviations from the usual structure was brought to me for dissection; with its history whilst alive, I am therefore unacquainted. The subject was a female infant, which measured two feet in length; the umbilicus was firmly cicatrized, and the umbilical vein closed; from these circumstances I conclude that it was about ten months old. The muscles of the child were large and firm, and covered by a considerable quantity of healthy fat; indeed the appearance of the body strongly implied that the child had, when living, possessed much vigour of constitution

I shall first relate those varieties of the sanguiferous system which were found on the thoracic side of the diaphragm, and afterwards describe those which were discovered in the abdomen; this will naturally lead me to the account of the uncommon state of the liver. The situation of the heart was reversed; the basis of that organ was placed a little to the left of the sternum, whilst its apex extended considerably to the right, and pointed against the space between the sixth and seventh ribs. The cavities usually called the right auricle and ventricle were consequently inclined to the left side of the body; therefore, to avoid confusion in the description, I shall, after Mr. WINSLOW, term them anterior, whilst those cavities usually called left, I shall term posterior. The inferior vena cava past, as usual, through a tendinous ring in the right side of the centre of the diaphragm, it afterwards pursued the course of the vena azygos, the place of which it supplied; after having united with the superior cava, the conjoined veins passed beneath the basis of the heart, to expand into the anterior auricle. The veins returning the blood from the liver united into one trunk, which passed through a tendinous aperture in the left of the centre of the diaphragm, and terminated immediately also in the anterior auricle.

The distribution of blood to the lungs, and the return of it from those bodies, were accomplished after the usual manner.

The aorta, after it had emerged from the posterior ventricle of the heart, extended its arch from the left to the right side, but afterwards pursued its ordinary course along the bodies of the dorsal vertebræ.

From the curvature of the aorta there first arose the common arterial trunk, which, in this subject, divided into the left carotid and subclavian arteries; whilst the right carotid, and subclavian, proceeded from the aorta by distinct trunks.

The inferior aorta gave off the cæliac, which, as usual, divided into three branches; however, that artery which was distributed to the liver appeared larger than common; it exceeded, by more than one-third, the size of the splenic artery of this subject. This was the only vessel which supplied the liver with blood, for the purpose either of nutrition or secretion.

The vena portarum was formed in the usual manner, but terminated in the inferior cava, nearly on a line with the renal veins. The umbilical vein of this subject ended in the hepatic vein.

The liver was of the ordinary size, but had not the usual inclination to the right side of the body; it was situated in the middle of the upper part of the abdomen, and nearly an equal portion of the gland extended into either hypochondrium.

The gall bladder lay collapsed in its usual situation; it was of a natural structure, but rather smaller than common; it measured one inch and a half in length, and half an inch in breadth. On opening the bladder, we found in it about half a tea-spoon full of bile; in colour it resembled the bile of children, being of a deep yellow brown; it also tasted like bile; it was bitter, but not so acridly or nauseously bitter as common bile.

I diluted a small quantity of this fluid with water, and with this liquor moistened some paper which had been tinged with a vegetable blue; this was instantly changed into a deep green, consequently this fluid, like common bile, abounded with alkali. I added some diluted nitrous acid to a small quantity of this, and of common bile; they both became changed, by this addition, to a similar green colour. The colouring matter of the bile therefore appears to have possessed its common properties.

The gall ducts had been divided, in removing the stomach and duodenum, before the uncommon termination of the vena portarum was discovered, and some bile had flowed from the divided ducts.

The intestines did not contain much alimentary or fœcal matter; this was, however, as usual, deeply tinged with bile.

The spleen consisted of seven separate portions, to each of which a branch of the splenic artery was distributed. The other viscera were sound, and of their usual structure and appearance.

No cause could be discovered to which the child's death could be assigned. We observed that the tongue was incrusted with a dark coloured mucus, which indicated the existence of fever previous to the infant's death.

When an anatomist contemplates the performance of biliary secretion by a vein, a circumstance so contrary to the general economy of the body, he naturally concludes, that bile cannot be prepared unless from venal blood; and he also infers, that the equal and undisturbed current of blood in the veins is favourable to the secretion; but the circumstances of the present case, in which bile was secreted by an artery, prove the fallacy of this reasoning. I extremely regret that only so small a quantity of this bile could be collected from the gall bladder; as, surely, it was very desirable to ascertain more fully how far the qualities of this curiously prepared fluid resembled common bile.

That the fluid secreted by the liver was not, in this case, deficient in quantity, appears to me sufficiently evident. If the gall bladder had not suffered occasional repletion, I think it would have been found in a state of greater contraction. Some bile had escaped from the divided gall ducts, and a considerable quantity of this fluid would be required to give so deep a tint, as in this case was visible, to the alimentary matter.

I cannot, therefore, but suppose that the empty state of the gall bladder was the effect of accident, and not of deficient secretion by the liver. The bulk and well nourished state of the body do, I think, demonstrate that there was no defect in the functions of the chylopoetic organs.

But it will surely be inquired, from what cause the death of the child originated. It may be suspected that the mal-formation of the liver contributed to its decease ; and particularly as no derangement of any vital organ could be discovered. Yet if it be considered how frequently children die from nervous irritation, or fever, the probability of this suspicion is, in my opinion, diminished. The circumstances of the case may impress others with contrary sentiments; I shall remain satisfied with having faithfully described the appearances of the body, and having offered those remarks which I believed deducible from them.

The peculiarity of the next case, which I have the honour to lay before the Society, consists in an uncommon formation of the alimentary canal. The body of a boy was brought to me for dissection ; it measured four feet three inches in length; it was well formed, and had moderately large limbs; they, however, appeared flabby, as if wasted by recent disease.

The abdomen was enormously swoln; which being opened, there appeared a more than ordinary extent of large intestines, in a state of great distention.

The diameter of the canal measured about three inches, and its dimensions were nearly equal in every part.

The matter with which it was turgid was of a greyish colour, of a pulpy consistence, having little fœtor, and quite unlike the usual fœcal contents of the large intestines.

The length of the colon was uncommon; having, as usual, ascended to the right hypochondrium, it was reflected downwards, even into the pelvis; it then reascended to the left hypochondrium, and afterwards pursued its usual course.

After turning aside this large volume of intestine, to examine the other parts of the alimentary tube, we were surprised to discover that the subject contained scarcely any small intestines. These viscera, with the stomach, lay in a perfectly collapsed state; their texture was extremely tender; they were torn even by a gentle examination. The duodenum, jejunum, and ileum, when detached from the body, and extended, measured only two feet in length, whilst the extent of the large intestines exceeded four feet.

The utmost length of the intestinal tube, in this subject, was little more than six feet, whereas it should have been about twenty-seven feet, had it born the ordinary proportion to the length of the body.

I distended and dried this curious alimentary canal, and still have it in preservation.

As the small intestines measured only two feet in length, this extent was doubtless insufficient for the preparation and absorption of chyle; these processes must therefore have been, in a great degree, performed by the large intestines.

The form and stature of the boy shew that nutrition was not scantily supplied; he died evidently from a want of intestinal evacuation. Whether the unusual structure of the canal contributed to the production of disease, cannot, perhaps, be readily determined; it appears, however, very probable that uncommonly formed parts, although capable of supporting life, may be less adapted to sustain the derangement of functions consequent to disease.

In Tab. VII. and VIII. are represented the appearances described in the first of the foregoing cases.

Tab. VII.

A. The anterior ventricle, which is usually inclined to the right side.

B. The anterior auricle.

C. The posterior ventricle, which is usually inclined to the left side.

D. The posterior auricle.

E. The superior vena cava.

F. The aorta.

G. The pulmonary artery.

H. The common trunk of the left carotid and subclavian arteries.

I. The right carotid.

K. The right subclavian.

L. The hepatic vein.

M. Part of the diaphragm.

N. The liver.

O. The superior mesenteric artery.

P. The renal artery.

Q. The renal vein.

R. The vena cava inferior.

S. The aorta continued.

T. T. The vena portarum.

Tab. VIII.

A. The anterior auricle, turned backwards, that the vena cava may be seen.

B. The posterior ventricle.

C. The posterior auricle.

D. The superior vena cava.

E. The inferior vena cava.

F. The conjoined veins passing beneath the basis of the heart to the anterior auricle.

G. The beginning of the vessels of the right lung.

H. The pulmonary artery.

I. The aorta.

K. The hepatic vein.

L. Part of the diaphragm.

M. The liver.

N. The cæliac artery.

O. The hepatic artery.

P. The splenic artery.

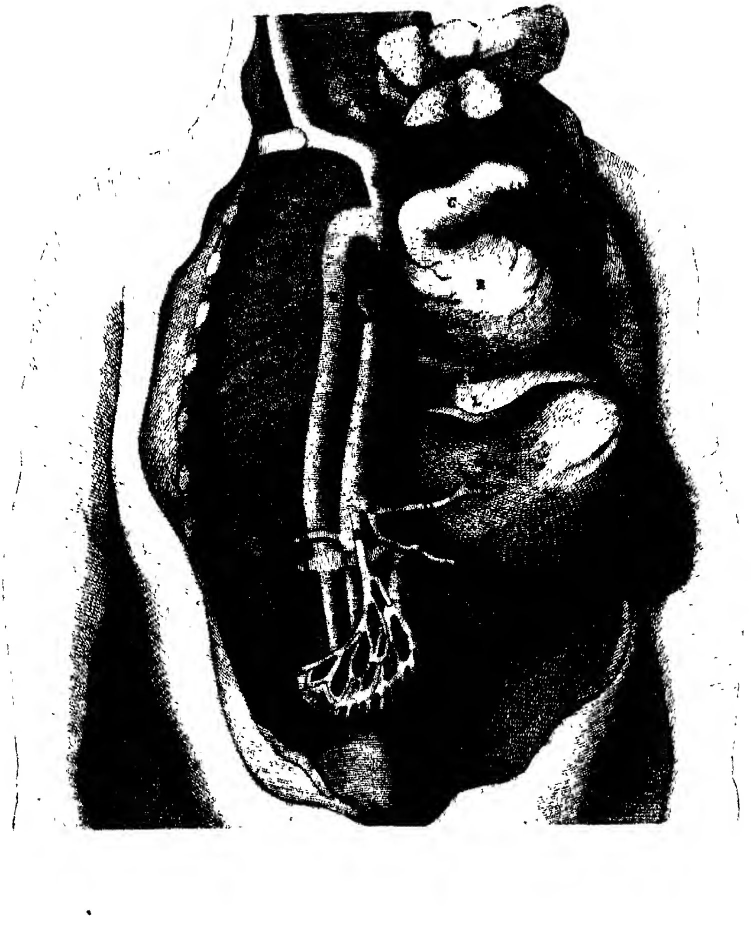
Q. The renal artery.

R. The superior mesenteric artery.

S. The renal vein.

T. T. The vena portarum.


D
A



X. *An Account of the Equatorial Instrument.* *By Sir* George Shuckburgh, *Bart. F. R. S.*

Read March 21, 1793.

> " Juvat ire per altum
> " Aëra, et immenso spatiantem vivere cœlo;
> " Hoc sub pace vacat tantum." MANILIUS, lib. i.

(1.) BEFORE I enter upon the description of the instrument which I propose particularly to describe, it may not be improper to say something of the equatorial in general, and of such instruments as, having been made upon a similar principle, have been used by different astronomers, in different ages.

The first account, that I meet with, of an astronomical instrument that bears any resemblance to it, is to be found in PTOLEMY, (lib. 5 of his Almagest) wherewith, he tells us, he determined the distance between the two tropics. This instrument is described under the name *αστρολαβικον οργανον*, and appears to have consisted of two circles, placed at right angles to each other, one representing the meridian or solsticial colure, and the other the zodiac; the former turning upon an axis, placed parallel to the axis of the earth, being elevated to the latitude of the place, and the other turning within it on two centres, removed $23\frac{1}{2}°$ from the former axis; and was in truth not very unlike the common ring dial, only about six times as large. Each circle was divided into 360°, and those again into three or four subdivisions, and being furnished (it may be supposed) with moveable sights, the observer was enabled to take the elevation or depression of any object above or below the ecliptic, together with its distance from the meridian, or colure, that circle being previously placed parallel to its corresponding one in the heavens. The first measure would give the latitude of any heavenly body, and the latter the longitude. This instrument, or something similar to it, seems to have been in use as early as the time of HIPPARCHUS, who lived in the second century before our Saviour, (vide WEIDLERI *Hist. Astron.* p. 319; et TYCHONIS BRAHE *Mechanica*) and was continued to be used by astronomers for upwards of fifteen centuries afterwards.

(2.) The next account that occurs is by J. MULLER, REGIOMONTANUS, sive JOANNES de Monte Regio, who flourished about A. D. 1460, and, in a posthumous treatise expressly on this subject, intitled *Scripta clarissimi Mathematici* M. JOANNIS REGIOMONTANI *de Torqueto, Astrolabio armillari, Regulá magná Ptolemaicá, Baculoque Astronomico, &c. &c.* in quarto, printed at Nuremberg in 1544, has given a pretty full account, not only of the armillary astrolabe, but also of the *torquetum*, which in fact was nothing more than a portable equatorial, and may be considered as the first instrument truly of this kind. As this treatise is become extremely scarce, and I know of only one copy in this kingdom, I take this opportunity of apprizing the curious, that it is to be met with in the British Museum. A short description, however, of the *torquetum*, with a plate of the instrument, will be found in Mons. BAILLY's *Astro-*

nomie Moderne, Tome I. p. 687; and a description of the *astrolabium armillare* of PTOLEMY, according to REGIOMONTANUS's conception of it, who may be considered as the best commentator upon the Almagest now to be met with, will be found in WEIDLER's *Historia Astronomia*, quarto, 1741.

(3.) The next author that presents himself is COPERNICUS, (who lived in 1530) and in his work *De Revolutione Orbium cœlestium*, lib. 2. c. 14. *De exquirendis Stellarum Locis*, professedly describes the same instrument with PTOLEMY; but, as it appears to me, something more complicated, having a greater number of circles, and in truth what in later times has been understood by the name, Armillary Sphere.

(4.) After COPERNICUS, I find, in a work of APIAN, who was his contemporary, or a little after him, viz. about 1538, a complete description of the *torquetum*, with all the parts of it minutely detailed, assisted by four or five wooden plates, together with the use of the instrument. This work, which is also very scarce, is in folio, intitled, *Introductio geographica* PETRI APIANI *in doctissimas* VERNERI *Annotationes, &c. &c. cui recens jam opera* P. APIANI *accessit Torquetum, Instrumentum pulcberrimum sane et utilissimum. Ingolstadii, anno* 1533. Towards the conclusion of this work is a curious letter of REGIOMONTANUS to Cardinal BESSARION, *De Compositione Meteoroscopii*, that is, the armillary sphere that was used by PTOLEMY, with a plate of it.

(5.) To APIAN succeeded, at some distance, but exceeded all that went before him, the justly celebrated TYCHO BRAHE, who in his *Astronomiæ Instauratæ Mechanica*,* *Noribergæ*, 1602,

* See also *Hist. Cœlestis, Lib. Prolegom.* TYCHONIS BRAHEI, *Augustæ Vindelicorum*, 1666, II. Vol. folio, p. 118 and 119.

folio, has given us a description and wooden plates, of no less than four different astrolabes, under the names of *armillæ zodiacales et equatoriæ*, of different sizes, from $4\frac{1}{2}$ to 10 feet diameter, divided into degrees and minutes, and some of them into every 15 or 10 seconds, but furnished only with plane sights. These large instruments were placed in towers appropriated to each, with moveable roofs, one half of which was taken away at the time of observation. A circumstance that it is curious to remark, is, that TYCHO, who was attentive to every thing that could improve the accuracy of his observations, made the axis of his 10 feet circle hollow, "Axis "ejus è chalybe constans, et undiquâque apprimè teres; in-"terius tamen *cavus*, ne pondere *officiat*, in diametro est "trium digitorum;" a principle that has been very prudently re-adopted in these later times, as will be presently seen.

(6.) After TYCHO I meet with no instrument of this sort till the time of CHRISTOPHER SCHEINER, about the year 1620, who made use of a small telescope, moving upon a polar axis, with an arc of 47° of declination, to observe the sun's disc commodiously, and examine his spots; an account of which will be found in his *Rosa Ursina*, folio, Bracciani, 1630, p. 347. But this instrument can hardly be considered as an astronomical one, being merely a contrivance to follow the sun with a telescope, by means of one motion only, similar in its object with the heliostate, described by Dr. DESAGULIERS, (Mathematical Elements of Natural Philosophy, lib. 5. c. 2.)

(7.) Again, FLAMSTEED'S sector, which he has described in the prolegomena to the third volume of his *Historia Cœlestis*, p. 103, though mounted upon a polar axis, and very ingeniously contrived for the purpose it was intended for, viz.

to measure the angular distances between the stars, having no divided circle at right angles to the polar axis, to take right ascensions, cannot come into the class of equatorial instruments. Nor need I here mention Mr. MOLYNEUX's telescopic dial, (*Sciothericum telescopicum*, in 1686) though depending upon the principle of a polar axis, which, like a ring dial, or equinoxial dial, was little more than a play-thing for an amateur in astronomy.

(8.) But about the year 1730 or 1735, when the practice of astronomy had assumed a new face in this kingdom, under the skill of Dr. HALLEY and of Dr. BRADLEY, Mr. GRAHAM invented his sector, for taking differences of right ascension and declination out of the meridian; and this may be considered as bearing a considerable affinity to the equatorial instrument in principle, and differing from it only in the extent of its powers. Of this instrument, which is well known to every practised astronomer, a complete account will be found in SMITH's Optics, Vol. II. § 885. and in Mr. VINCE's Astronomy. I approach now to the period when the modern equatorial instrument, properly so called, took its origin.

(9.) Mr. JAMES SHORT, a person of very considerable eminence for his skill in the theory and practice of optics, and particularly for the unexampled excellence to which he had carried catoptric telescopes, in which, I believe, he has never yet been exceeded: Mr. SHORT, I say, probably finding himself capable of making telescopes, of very moderate dimensions, fit for many astronomical purposes, and able to exhibit several of the heavenly bodies by daylight, provided they were furnished with a convenient apparatus and movement for that purpose, applied a two feet reflecting telescope, for the first time, to a combination of circles, representing the horizon, the meridian, the equator, and moveable horary circle, or circle of declination, each divided into degrees, and every third minute, furnished with levels, &c. for adjustment to the place of observation. This machine was invented in or before the year 1749, and is described in the Philosophical Transactions for that year. But as this instrument was furnished with no counterpoises in any part, and the length of the telescope (two feet) was found considerably too great for circles whose diameter was not more than six inches, it became unsteady, and unfit for any other purpose than that of finding and following a celestial object, and, on account of its high price also, was, as far as I believe, but little made use of.

(10.) However, after some years had elapsed, the idea of an equatorial telescope was again renewed by three several artists in this kingdom, Messrs. RAMSDEN, NAIRNE, and DOLLOND, with many and very material improvements, such as to carry the portable equatorial almost to perfection. Of this instrument Mr. RAMSDEN had made three or four, as early, I believe, as the year 1770 or 1773; viz. one for the late Earl of Bute, one for Mr. M'KENZIE, another for Sir JOSEPH BANKS, and lastly, one for myself; with which I made a great many astronomical and geometrical observations in France and Italy, in the years 1774 and 1775, some of which may be seen in a Memoir upon the Heights of some of the Alps, printed in the Philosophical Transactions for 1777. Of this machine a plate and description in French was printed in the year 1773, and reprinted in English in 1779. An ample account of this equatorial will be found in Mr. VINCE's Treatise on practical Astronomy, p. 152.

In 1771 Mr. NAIRNE published an account of his equatorial telescope, in the Philosophical Transactions for that year; and in 1772 or 1773 Messrs. P. and J. DOLLOND printed an account of theirs. Each of these instruments were furnished with counterpoises, and, in general principles, were at least similar, if not the same. The preference that I was inclined to give at that time to my own instrument, made by Mr. RAMSDEN, was owing to the peculiar advantage of a swinging level, to the unexampled accuracy of its divisions, and its great portability. If, in what I have just now said of the three last instruments, I should have committed any error with respect to the priority of their improvements, I must leave that point to be settled by the artists themselves, and shall hasten to the description of the instrument I set out with. But first one word with respect to an instrument that has been in frequent use on the continent, called, very absurdly, a Parallactic Machine.

(11.) The first notice, that I find of it, is in the History of the Academy of Sciences at Paris, for 1721, p. 18, in a memoir of Mr. CASSINI, with a description and plate of it; also in the History of the same Academy for 1746, p. 121, wherein it is said to have been proposed by Mr. PASSEMENT, but without any description of it; it will, however, be found described, with a plate of it, in the *Dictionnaire de Mathematique, par* Mr. SAVERIEN, two vols. quarto, 1753; and this account has been copied into OWEN's Dictionary of Arts and Sciences, in four vols. octavo. It appears to have been a frame of wood supporting a polar axis, with an equatorial and declination circle, of only a few inches in diameter; and was in fact no more than a very bad stand to a refracting telescope of 8 or 10 feet long, giving it a motion *parallel* to the equator; and hence some person, not very learned, gave it the name, *Machine parallactique*, as if παραλλακτος and παραλληλος were the same word. It is true that the early astronomers did use a machine called *Regulæ parallacticæ*, but that was an instrument to take the altitudes of the moon, and from thence to determine her parallax. Nor can I say much in favour of a machine of the same name, described in Mr. LA LANDE'S *Astron.* Vol. II. § 2004, which certainly does not do a great deal of credit to the state of the mathematical arts amongst the French; it however may have its convenience, as it is probably attainable at a very small expence. The author last mentioned speaks (§ 2409) of an equatorial in his possession, made by one VAYRINGE in 1737, with circles of 7 or 8 inches diameter, but of what construction we are not informed; and the name of the artist is, I confess, totally new to me. An instrument also of this nature, made by MEGNIE, for the President DE SARON, is described, and seems to be well imagined for a portable machine. This very amiable and ingenious gentleman, Mons. DE SARON, was so obliging, amongst other civilities when I was at Paris in 1775, to shew me a small reflector upon an equatorial stand, with some wheel work to keep it constantly following a star, together with an apparatus for the refraction, altitude, and azimuth, if I recollect right; and in the year 1778 Mr. WILLIAM RUSSEL, a late worthy member of the Royal Society, shewed me a small instrument of the same kind, that had been made by the late Mr. BIRD.

(12.) From the preceding account, it must appear that the equatorial instruments hitherto made, either from the smallness of their dimensions, or defect of their constructions, were totally unfit for the accuracy of modern astronomy, where an error of a few seconds only, in an observation, is all that can be admitted, to entitle it to any credit.* With respect to the precision of astronomical instruments in general, I may notice by the way, that from the time of HIPPARCHUS and PTOLEMY, before and at the commencement of the Christian æra, to the age of WALTHER and COPERNICUS, in the beginning of the 16th century, few observations can be depended on to within less than 5, 8, or perhaps even 10 minutes; those of TYCHO BRAHE, indeed, that princely promoter of astronomy, to within one minute. The errors of HEVELIUS's large sextant of 6 feet radius, towards the middle of the last century, might amount to 15 or 20 seconds. FLAMSTEED's sextant to 10 or 12 seconds; and lastly, those of Mr. GRAHAM's mural quadrant of 8 feet radius, with which Dr. BRADLEY made so many observations from 1742, might amount to 7 or 8 seconds.

(13.) Having said thus much generally upon the subject of this ingenious instrument, and not more, I trust, than will be deemed, by every lover of this science, what its importance deserves, I proceed to the description of one I have caused to be made by a very able artist of this metropolis, Mr. JESSE RAMSDEN.

AB, CD, EF, GH, (Tab. IX.) are 4 columns composed of hollow brass tubes $3\frac{1}{2}$ inches in diameter, and 5 feet 10 inches long; these, with two others, one of which appears

* I must except from this remark the two large equatorial sectors made by Mr. SISSON, for Greenwich observatory; and also an instrument of this kind, made by Mr. RAMSDEN, for the late General ROY, and now in the possession of Mr. AUBERT, whose circles are about 30 inches in diameter.

in part at IK, and the other wholly hid behind EF, are firmly fixed, at their upper ends, to a circle of bell metal, BDFH, and, at their lower end, to an inverted truncated hollow cone* LLL, of brass, in height 2 feet, and in diameter at its base AG, 1 foot 9 inches. The cross pieces or tubes P, P, as likewise O, O, and O, O, serve to connect the columns more strongly together, and prevent their bending. These several parts constitute the principal axis of the instrument, the lower end of which terminates in a steel point or cone, resting in a hollow conoid of bell metal, in such manner that the apex of the former does not reach to the bottom of the latter, but the place of bearing, and of friction, is (it may be) about two-tenths of an inch from the extremity of the cone; the other end of this axis finishes in a cylindrical pivot N, of about 1¼ inch long, and 1 inch in diameter, turning in a Y of bell metal. The entire length of this axis is 8 feet 4 inches, the lower end being supported by an iron frame 3, 4, 5, 6, 7, 8, which is firmly fixed, below the floor, into brick work, and, by means of two iron bars, one of which is seen at 28, and the other on the opposite side, not visible in the drawing, is kept secure from any motion eastward or westward; the lower part of this frame, to about one foot high, is inclosed by a mahogany box, or case, 9, 10, the top of which is intirely covered up, and serves as a die or base to this end of the instrument. The other extremity of this long

* Upon this cone are inscribed the following words.—"Hocce Panorganon Uranometricum à Jesse Ramsden, Londinensi Optico celeberrimo, et omnibus id genus artificum longe anteponendo, excogitatum, decem post annos nunc tandem absolutum, Georgius Shuckburgh Baronettus, in testimonium amoris sui erga res astronomicas, et ad easdem promovendas, fieri curavit, anno 1791."

axis, viz. the pivot N, rests upon the strong iron support 29, 30, 31, standing ten feet above the floor, made of massive pieces of cast iron, 2½ inches wide, and ½ inch thick, and held firmly together by bolts and nuts, as in the figure. 32, and 33, are two iron bars nearly at right angles to each other, and at ½ right angles to the meridian, that connect this upright support with the walls of the building, and, going through the bricks, are held fast by iron collars and nuts on the outside of the wall; these bars or braces resist any tendency, from the weight or pressure of the instrument, to push the supporter 29, 30, and 31, out of its upright position; and, being at right angles to each other, serve to keep it steady with respect to any lateral force that may accidentally be applied. The lower part is continued below the floor, and firmly fixed, with mortar and lead, into the brick work of the arch which will be presently described. The bottom is shut up in a box or plinth of mahogany, 34, 35, as has been mentioned in the description of the frame supporting the other end of the axis, at 9, 10. Near the lower end of the principal axis LN, are inserted 10 concentric brass cones or radii, *aa*, *bb*, *cc*, *dd*, *ee*, carrying on their extremities a graduated brass circle, of 4 feet diameter,* at right angles to the principal axis already described; this circle has two sets of divisions, one of points, and one of lines, each into degrees, and every 10 minutes, and the intermediate minutes and seconds are read off by the microscopes W and X, with a moveable wire and micrometer screw, such as has been described in General Roy's Account of his Instrument for measuring horizontal Angles. (See Philosophical Transactions, Vol. LXXX. p. 145.). The circle just

* More correctly 49½ inches.

mentioned is inclosed by a circular frame, or rail of mahogany, 14, 15, which is supported by ten balusters, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, and serves to protect the brass circle from any accidental injury in passing by it, without depriving it of exposure to the general temperature of the room. It, at the same time, affords means of support to a small lamp, 13, which, by reflection from the perforated speculum at the bottom of the microscopes, to be seen in that marked X, throws light upon the divisions by night. 26, 26, and 27, are iron rods, that, by being attached to the wooden case, 9, 10, give steadiness to the upright balusters, and the circular frame that they support. 1, and 2, are large stout brass cones, firmly fixed into the frame 3, 4, 5, 6, 7, 8, before mentioned, and whose use is to carry the microscopes W, X; any degree of pliancy or flexure in these cones would be readily discernible in the microscopes, and extremely detrimental to the observations, they are therefore made as stiff as possible. γ, δ, is a plane forming the upper side of the frame 3, 4, 5, 6, &c. and consisting of 3 plates, two moveable in grooves, and one fixed, furnished with suitable screws, one giving the extremity of the axis a motion upward or downward, and the other a motion to the right or left: this latter is procured by a rod passing through the cone 2, one end screwing into the plate below L, near the centre, and the other turned by an occasional handle fixed on near X; the former motion, viz. of elevating or depressing the axis, is procured by a handle fixed on to a screw near ϵ. QR is another circle, of the same dimensions with the former, graduated in the same manner, and held together by eight conical radii, firmly screwed to a circular centre piece, which serves as a base to a large conical axis, 2 feet 3 inches long, one side of which is seen at U, and its exterior extremity near V, with its sliding plate and screws for adjustment. Close behind the graduated circle, and at right angles to this axis, passing through it, lies the telescope TS, $5\frac{1}{2}$ feet long. This circle is likewise furnished with two microscopes, and micrometers, as in the equatorial circle, one of which is seen at full length at Y and Z, the eye tube being at Y, and the object glass, with the perforated speculum to throw light, at Z; the other microscope, on the opposite side of the circle, is not so discernible in the drawing, being completely foreshortened at Z near T, the eye of the draftsman having been exactly in the axis of the tube of the microscope. $\alpha, \alpha, \alpha, \alpha, \alpha, \alpha$, is an hexagonal lozenge, composed of six brass rulers, firmly fixed to the columns AB and EF, and supporting the lower end of the microscopes, as the pieces $\beta\beta$, $\beta\beta$, in like manner, sustain the upper end. By these means the wire in the field of the microscopes becomes a fixed immoveable index, and, after proper adjustment, an exact diameter of the circle, whilst the telescope, together with the circle, turns round the conical axis before mentioned. At P is a spirit level, passing through the centre plate of the conical axis at right angles to the telescope, supported by a cock at each end, one of which appears at *k*; this cock is fixed to the cone U, and, by means of a small toothed wheel and pinion, the level is made to revolve round its own axis, so that the same side of the level may readily be brought uppermost, whatever position the circle be put into; it is also furnished with all necessary adjusting screws. It will readily be seen that a telescope, thus fitted up, will have all the properties of a transit instrument, while the graduated circle will possess those of a meridian quadrant. For this purpose *l, m,* is a stout brass tube, inclosing a stiff iron rod, turning upon two fine steel points, adjusted, by proper screws, parallel to the line of sight of the telescope; this rod is attached to a spirit level of great sensibility, lying below it, which, with the rod, turns round upon the steel points just mentioned, and is in fact a hanging level of the best construction. At the eye end of the telescope S, is a peculiar apparatus to correct the effects of refraction and parallax, when an observation is made out of the meridian: it is composed of two levels, a small quadrant of altitude *n, o,* and a semicircle divided, with its nonius, to every 5′, on the breech plate of the telescope, the exterior eye tube having a circular motion, by a wheel and pinion at *o,* independent of the tube that carries the cross wires; by this means, the angle of the horary and vertical circles may at any time be found, together with the altitude of the object, and then, by the resolution of two right angled triangles, the refraction and parallax, in right ascension and declination, will be obtained. *t, u,* are two handles to a Hook's joint at *x, x,* which, turning an endless screw at *w, w,* give a gradual motion to the telescope in right ascension or declination; and this motion can at any time be restrained by a clamp at *q*. The handles *t, u,* are hung on to any part of the instrument, by means of the line and wire *v, v,* and are thus kept within the observer's reach. *r* and *s* are two microscopes, placed on opposite sides of the circle QR, and at right angles to the line of sight of the telescope, of use only when the plumb line is used in preference to the level *l, m,* above described, either for adjusting the instrument, or observing a meridian altitude. *y* and *z* are thin perforated brass plates, attached to the cover that goes on the object glass, and, by occasionally turning them over it, change the aperture to $\frac{1}{2}$ or $\frac{1}{4}$. The cross wires, of which there are 3 vertical, and 1 horizontal, within the eye tube S, have all the requisite adjustments by screws, &c. as in a common transit instrument, and are enlightened by night, by a lamp fixed near to one end of the declination axis U, viz. that opposite the end V; but this part of the apparatus is hid behind the axis and the telescope, except the weight *i*, which is a counterpoise. This lamp throws a light through the conical axis, which is perforated at that end on purpose, on a speculum in the centre of the telescope, placed at $\frac{1}{2}$ right angles to the axis of the object glass, and from thence reflected to the cross wires. This speculum, which is an elliptical diaphragm, is perforated to permit all the rays from the object glass to pass unobstructed to the eye. This contrivance has been mentioned by Mr. VINCE. (Practical Astronomy, p. 80.) From what has been already described, it must now be evident that if the principal, or polar axis, as it has been called, LN, be elevated to the latitude, and adjusted to the meridian of the place, if the line of sight of the telescope be at right angles to the axis VU, and this latter at right angles to the polar axis LN, the brass circle 14 and 15 will correspond with the equator in the heavens, and the circle QR will become an horary circle; viz. that if the centre of the wires, in the field of the telescope, be directed to any celestial object, on QR will be had its declination, and on 14 and 15 its distance from the meridian, from whence, by knowing the hour, the right ascension will be obtained,* and consequently its true place in the heavens.

(14.) Before I proceed, it may be necessary to say something of the remaining parts of this apparatus, such as are either necessary to it, or concomitant in the use of it. These are, 1. The lamp to illuminate the cross wires. 2. The refraction piece. 3. The plumb line. 4. The moveable roof. 5. The regulator. 6. The meridian mark. And these will be best understood by inspection of the plates, where Tab. X. figure 1, represents the lamp fixed to the farther end of the declination axis. AB is the brass case or lantern, suspended on two centres C and D, within the frame E and F, which is attached to the pillars of the equatorial (IK, GH, and that one hidden behind EF) by the cylindrical braces *a, b, c, d*. *e* is the lamp or vessel containing the oil, swinging upon the centres *f* and *g*, at right angles to C and D; and by means of this cross axis, and the counterpoise *b*, the lamp is kept constantly in an upright position, whatever may be the situation of the declination axis. G is a chimney to carry off the smoke from the instrument, and prevent its heating it. *i*, is a convex lens, that collects the rays from the flame upon the extremity of the declination axis, U, V, which, being hollow, conveys *all* the light to the perforated speculum before mentioned, within the centre of the axis and of the telescope; and this is assisted by another convex lens at the end of the axis, before which occasionally is screwed a pale green glass, of which there are three, of different shades, to temper the light of the lamp, if necessary, to the light of the

* The adjustments which I have just now mentioned, together with some others, will be duly explained in their proper places.

star that is to be observed. This light, as was said before, being reflected down the tube to the wires, the stars are seen upon a beautiful pale green field, the wires appearing black. The centre D being of necessity full two inches in diameter, in order to admit of the aperture and lens *i*, rests on three friction wheels, and by this means the motion is as easy as on the centre C. At *k*, *l*, *m*, *n*, is a small sliding door, to close up the lantern from the wind, but which is here removed to shew the inside.

(15.) Tab. X. figure 2, represents the refraction piece. AB is a portion of the telescope; C the eye tube; *a*, *b*, *c*, a divided semicircle; *d* its nonius fixed to AB, shewing the angle of the horary and verticle circles; *e*, a small spirit level, attached to the plate on which this semicircle is engraved, and moving with it by means of the screw *f*, which turns a pinion, that works in a toothed wheel, that turns the whole plate, together with the exterior eye tube, round its centre, but without moving the tube that carries the cross wires. From hence it may be understood, that by turning the screw *f*, till the level *e* stands true, the index *d*, which represents a point in the horary circle, will mark how much the division zero, *(o)* which represents the vertical, is inclined thereto. *l*, *k*, is a small quadrant of altitude, that, by means of the level *g*, and screw and pinion *h*, turning on a centre at *m*, gives the elevation above the horizon of any object in the field of the telescope. *i*, is a small aperture through which a key is fixed on, to give a lateral motion to the wires to adjust them; and near *f* is another screw, to adjust them parallel to the equator and declination circle.

(16.) Tab. X. figure 3, represents the plumb line and its frame; it is about 5 feet long, and suspended to the roof of the observatory by the two hooks *a, a*. AB is a hollow brass tube, to protect the plumb line from the wind; the line or wire is fixed at the top near *b*, and sustains the plummet in a glass of water at C. *b* and *c*, are two adjusting screws, moving a sliding plate at right angular directions, to which is fixed the upper end of the plumb line. *d* is a screw, that by means of a pinion moves a rack *e, e*, to depress or raise the glass of water, and thereby support the plummet, (when this apparatus is moved about from one side of the instrument to the other) and prevent the wire being broken, and, by depressing it, to enable the plummet to play free, when it is to be used. At A and B are two apertures, at 4 feet distance, corresponding with the two microscopes, covered on the backside by pieces of transparent ivory, in order to exhibit an agreeable field of view, to observe the coincidence of the plumb line with the cross wires in the microscopes.

(17.) In Tab. XI. is seen a section of the building in which this instrument is placed. As the equatorial is a machine calculated to observe the heavenly bodies in every part of the hemisphere, so it became necessary to construct a chamber, with windows opening to every quarter, north, east, west, and south; and to any degree of elevation above the horizon. With this view I erected a building, or small turret, within my house, at Shuckburgh, in Warwickshire, see Tab. XI. where *a, b, c, d*, represent a section of this room, being about $15\frac{1}{2}$ feet square, containing the equatorial AB, resting on its supporters CD. *s, o, p, r, q, t*, is a hollow conical roof,* moveable round its axis upon six friction rollers, of about 4 inches in diameter, two of which are seen at *s* and *t*. The base of this cone consists of an iron ring, about 11 feet in diameter, and 3 inches wide; the upper part of the cone at *s*, is terminated by another iron ring $2\frac{1}{2}$ feet diameter; and these are connected by 12 iron ribs, or rafters, in the direction of *s, s*. Over these rafters lie two coats of extremely thin deal planks, not more than $\frac{1}{8}$ inch in thickness each, crossing each other in transverse directions of the grain of the wood, and over these a covering of copper, of about the thickness of a shilling. Over the copper, on the outside, are three good coats of white paint, and the wood withinside is lined with stout canvas, well painted also; so that the whole roof is as moveable and as light as possible, not more than 200 or 300lb. weight, and withal very strong. At *s* and *s*, is an aperture in the roof, of about one foot wide, which is opened or covered, suitably to the occasion, by the two doors *o, p*. *r* is another door, (in the plate) open; by means of these three doors, all opened very readily by iron rods withinside, a prospect is given of the heavens from the horizon to the zenith, and even to 10° beyond it. *q* is another aperture, about 9 inches long and 4 wide, covered occasionally by a shutter *z*.

* As I flatter myself that I have now executed what the very celebrated Abbé Boscovich seems to have had in view, in the 14th opusculum of his fourth volume of *Opera pertinentia ad Astronomiam, &c.* I cannot restrain myself from citing the passage, wherein, after describing the use of the *small* equatorial, he says; "Apparet "igitur egregius usus machinæ etiam *mobilis*. Verum machina parallactica metallica "cum circulo, et semicirculo, satis magnis, ac telescopio acromatico, et satis bono mi-"crometro filari, collocata firmiter in turri habente tectum mobile, esset instrumentum "usus immensi et expeditissimi, ac incredibilis ad astronomiam cum maximo fructu "excolendam utilitatis." p. 309.

This little window affords a constant view of the pole star in its whole revolution, if the sky be clear, and consequently an opportunity of comparing a transit and altitude of any star to the southward directly with it; and, by moving the conical roof round, any part of the sky may be exposed to the telescope. The rollers* *s*, *t*, roll upon a surface of lead, melted and cast into a circular groove in the timbers at EE, and planed truly horizontal. Besides the apertures already described in the roof, the room is enlightened by two windows to the south-east, and north-west; and also by two oval lights in the side of the cone, nearly east and west, and by a third in the zenith at *r*. *v*, *w*, is a platform on the outside of the cone, covered with lead; of use in examining the roof, and cleansing it in time of snow, &c. *y* and *x* are iron balusters, very necessary on such occasions, with the iron rail at top at *x* placed obliquely, in a plane tending to the centre of the instrument, in order that the least possible light from any object may at any time be lost to the object glass. C is one of the iron bars passing through the wall of the building, fixed by a collar and nut on the outside, and resisting the pressure of the instrument, whose weight, without the supporters, is full 300lb. against the support D. *e*, *f*, *l*, and *g*, *h*, *n*, are two of the side walls of the building, rising 40 feet above the ground, and serving as *butments* to the arch *l*, *m*, *n*, which is farther strengthened by a square frame, of heart of oak timber, 9 inches by 6 inches, let into the wall at 5, 6, held together by iron bolts, and going round the building.

* Besides these rollers, whose axes are horizontal, there are 3 or 4 other rollers, upon a vertical axis, exterior to, and bearing against the ring that constitutes the base of the cone; by this means they keep the centre of its motion always in the same place.

This arch is filled with solid masonry * to the height *i, k*, into which, as upon a plane brick floor, the iron frames that support the instrument are firmly fixed with plaster and lead at 1, 2, and 3, 4, and hence the instrument seems as steady as upon a rock. *b, d*, is a boarded floor, that has no connection whatever with the instrument, or its supporters. *u, u*, are the two mahogany cases that cover up the apertures in the floor, and serve as bases to the supporters C and D, but do not touch the iron frames within $\frac{1}{8}$ inch; so that any yielding of the floor, *b, d*, by passing to and fro thereon, communicates no motion whatever to the instrument.

(18.) To the north-east wall † *c, d*, by means of timber laid into it, and long screws, is firmly fixed the clock, or sidereal regulator, but so as to have no communication with the floor. As it is peculiar in its construction, it may deserve some notice. Whereas most astronomical clocks shew sidereal time in hours and minutes, which is afterwards, in the course of computation, reduced to degrees and minutes; this machine shews the degree and minute of the equator, that is upon the meridian at any given instant, directly without reduction. This is of considerable convenience in observations, out of the meridian, with an equatorial instrument; inasmuch as the equatorial circle and the clock, by these means, speak the

* The bricks of this arch were laid dry, and then grout, consisting of gravel and hot lime, was poured upon them; when the arch had stood two years, the haunches were filled up with bricks, laid in mortar. I mention these circumstances, as attention to them may be of use upon a similar occasion, whenever it shall be necessary to erect an arch bearing so great a weight, viz. near 30 tons, upon such slight buttresses.

† The walls of this room do not correspond with the cardinal points; the section here given having an aspect 53° S. east.

same language. For this purpose the vibrations of the pendulum are only $\frac{2}{3}$ of a common pendulum, $= 10''$ of space; and the index that is carried round immediately by the pendulum, viz. on the same arbor with the pallet wheel, in one revolution describes $10'$ of sidereal space; the next index from the centre of the dial plate shews the degrees, and every 10th minute, making one revolution in $10°$. And lastly, the decades of degrees, from 1 to 36, $= 360°$, are shewn through an opening in the dial plate, which in some clocks is appropriated to the day of the month. But perhaps this will be better seen from inspection of the plate (Tab. XII.) wherein the hands are so placed as to indicate $147° 14' 10''$. The small hand (it may be perceived) has two sets of figures round its circle, the one shewing the minutes, and every 10th second, the other the number of beats from 0 to 60, and this latter enumeration is what alone can be attended to in the observations; that is, the degrees and every 10th minute are set down in the journal immediately from the indexes, and the subdivisions below $10'$ are set down in beats of the pendulum, reckoning from 0 to 60, and reduced afterwards at leisure. The pendulum, only 17 inches $\frac{4}{10}$ in length, is a compound gridiron composed of 5 rods, of which 3, viz. the centre and two exterior, are of iron, and the two next to, and on each side of the centre, are compounded of silver, brass, and zinc. The weight of the pendulum ball is about 6 pounds, and that of the clock weight 32. The spring, by which the pendulum is suspended, is said to be so constructed as to produce cycloidical arcs of vibration; but my reliance upon this contrivance is not very perfect. The two chief arbors are jewelled, the pallets are rubies, and the axis of the principal great wheel moves on friction wheels; and I believe every care has been taken, that the experience of one of the first artists of this kingdom* could suggest, to render it a complete piece of mechanism. It goes five months with once winding up; and, from the experience I have hitherto had, does not seem to vary its rate between winter and summer more than equal to three seconds *per* day, sidereal time.

(19.) All, I believe, that remains for me to mention, in this part of the account, is the meridian mark. (See Tab. XIII.) A, B, C, D, E, is a solid brick pier, erected at the distance of 2970 feet from the centre of the observatory; 8 feet high, 9 feet wide at bottom from A to E, and 4 feet wide at top from B to C; 1 foot 6 inches thick at top from C to D, and 2 feet 3 inches at bottom. F is an iron box, 8 inches square, and 1 foot high, exclusive of the top, or chimney; within this box is placed one of ARGAND's patent lamps, which shines through a circular aperture, of about $1\frac{1}{4}$ inch, in the front of the box, and exhibits the appearance of a fixed star to the naked eye by night; but the aperture being covered by a semitransparent piece of glass, ground rough on one side, transmits a steady uniform light, that through the telescope resembles the disc of a small planet $7''\frac{1}{2}$ in diameter. By making the wire in the telescope, which is only $2''\frac{1}{2}$ in diameter, bisect this circular light, the instrument may be very nicely adjusted to the meridian, indeed with so great precision, that I think an error of one second cannot be committed. Around this glass aperture, which by day appears perfectly black, is painted a circle of black, (on a ground of white) whose external diameter is 3 inches, and

* Mr. JOHN ARNOLD.

consequently subtends an angle of 18″. The bisection of this last is made use of by day, and although, it is true, it is rather larger than is necessary for a meridian *point*, yet it was convenient it should be so, to render it visible* in the dark winter months, if there was the least vapour rising from the ground. Yet with all this, no error need be apprehended exceeding 2″, equal to only $\frac{1}{8}$ of a second of time, nor that, unless the image should be greatly agitated by the vapours. This mark is not only of use to find the meridian, but also to determine the horizontal point, when once the angle of it above or below the horizon is ascertained; and, by comparing the meridian altitude of any object with this point, its declination may be had, almost as nearly as by the level or plumb line.

I ought to have mentioned before, that the box F is moveable upon the plane B, C, by means of an iron frame on which it slides, and screws to adjust it finally to the true meridian.

Although there can be no great danger of a solid pier, like the above, erected on a foundation four feet deep, being likely to move, yet this doubt, if any should occur, may be examined by a plumb line *a*, *b*, suspended from *a*, and playing near a corresponding mark, on the top of a post driven into the ground at *c*. G, E, are steps to ascend to light the lamp, or take it in and out at the door *d*. The whole is surrounded by high pallisadoes, to protect it from the accidents of cattle, or curiosity.

(20.) Having thus particularly described the various parts of the equatorial, I trust in a manner sufficiently intelligible to any person a little conversant with astronomical instruments, it may be proper to say something of its precision;

for without some competent knowledge of the accuracy of its parts, it is in vain to attempt to adjust them, and still more so to use them with any degree of satisfaction. And here the chief objects of inquiry have been the accuracy of the divisions; the sensibility of the levels; and the power of the telescope. I shall begin with the levels; and first with the axis level, P, *k*, the parts of which may be seen at figure 1, Tab. XIV. where 1, 2, represent the extremities of the axis of the circle of declination; A, a section of the tube of the telescope; 3, 3, the level tube, resting in the cocks or supporters, 4, 4, and 5, 5, and turning on an axis *d, e*; on one end of which *e*, is a toothed wheel *g*, moved by a small pinion *f*, turned by the screw head *b*; and at *a*, is an adjusting screw to bring the level parallel to the axis 1, 2; at *c* is another, to bring the level parallel to its moveable axis *d, e; b*, is an adjusting screw, at right angles to C, to bring the tube of the level parallel to its axis *d, e; i*, is another screw, to make this axis parallel to the axis 1, 2. All these, as well as every other adjusting screw throughout the instrument, have chamfered heads, with their circumferences divided into 10 parts, and the value of each ascertained; so that, by turning the screws a whole, or any part of a revolution, the angle of motion given to any part may be known. The use of this is obvious; but the degree of convenience is only known to such persons as have been conversant with these nice adjustments. *k* and *l*, are two moveable indexes, to be set to each end of the bubble. The tube of this level is about 14 inches long, and the curvature of it such, as that by giving it an inclination of 15″, the bubble moves about $\frac{1}{10}$ inch, and the third or fourth part of this space is plainly discernible, so that no error exceeding 3 or 4″ need be apprehended, if proper care is taken.

The swinging level *l*, *m*, fixed to the tube of the telescope, for the purpose of taking declinations, is unground, and of very superior sensibility, as it ought, having been selected expressly from a great many yards of glass tube, in order to get one with a proper flexure; and such has been the success of this care, that a change of inclination of this level of one single second, moves the bubble nearly $\frac{1}{6}$ of an inch, certainly more than $\frac{1}{10}$.* This, I am inclined to believe, is the greatest sensibility that has ever yet been attained by a level: it is at the same time somewhat inconvenient, since it sometimes will require two or three minutes time to settle to its true point, it moves so slow. An error, however, of more than a second need seldom be apprehended, which I conceive is as much as could have been expected from a plumb line of a length equal to the diameter of the circle. The bubble of this level is about 7 inches long, but this varies with the temperature; and I have experienced that with about 28° of heat the bubble contracts one inch in length; this makes it very necessary to be attentive to the index at each end of the bubble. The parts of this level may be seen at figure 2, Tab. XIV. A, B, is one side of the tube of the telescope, whereon are firmly fixed two upright supporters, or cocks, C, D, and E, F;

* If the motion of the bubble be taken to be even 0,2 inch, it will be found that the radius of curvature is more than 1100 yards; and, supposing the length of the level = 12 inches, it will only comprise, from one end to the other, an arch of 1′, and the versed sine of 30″, in this case, would express the depressure of the tube at its extremities below the centre of the level = about half $\frac{1}{1000}$ of an inch.

The curious ground level, for adjusting the axis of the transit instrument at the observatory at Greenwich, moves, as I have been informed, about $\frac{1}{10}$ inch for 1″.

in which the level G, H, I, about 12 inches long, swings upon two conical centres *a*, *i*, truly turned, of polished steel. G I, is an hollow brass tube, about $\frac{1}{2}$ inch in diameter, containing within it a steel triangular axis, one extremity of which is terminated by the conical centre *a*, and, by means of a spiral spring winding round the axis, is made to contract its length by pulling the trigger *g*, towards I, by which means the point or centre *a*, retires within the tube G I; so that the two centres may easily be released from the steel pivots wherein they play, and the level may be reversed, if necessary, to adjust it. It is evident that the axis *a*, *i*, should be parallel to the line *b*, *b*, which may be taken for a tangent to the curvature of the tube at *k*, and this is procured by the capstan headed screw *c*; it is also necessary that this same axis should be parallel to the line A B, or rather to the line of collimation of the telescope, in a north and south direction, and this is obtained by the capstan screw *d*. Finally, it is expedient that this axis be parallel to the line of collimation, in a direction east and west; and this is effected by the screw *h*. *e*, *e*, are two indexes to mark each end of the bubble, and, being fixed to the two concentric sliders *f*, *f*, which embrace $\frac{2}{3}$ of the circumference of the tube G I, are moveable any where at pleasure. *h* is one of two screws, the other being opposite and out of sight, for adjusting the axis *a*, *i*, parallel to the line of collimation, eastward or westward, as has been said, and at right angles to the declination axis. I proceed next to the divisions.

(21.) The superior advantages of an entire circle,* over a

* Vide *Observationes Astronomicæ Annis* 1781, 1782, 1783, *institutæ in Observatorio Regio Hauniensi*, auctore THOMA BUGGE, Hauniæ, 1784. Quarto. Cap. 5.

quadrant or sextant, for astronomical purposes, are almost incredible to a person that has not considered the properties of a circle, and so great, that I am much surprised it has not been brought into more frequent and earlier use. For, in the *first* place, the error of the centre, which so constantly takes place in quadrants, is entirely done away; *secondly*, the small error in each individual division is discovered, so that whatever be the skill of the artist, the observer is under no obligation to rely upon it, but can examine the whole himself. The method I proposed was this; to make the index wires in the opposite micrometers an exact diameter to the circle, and then to observe whether each division corresponded with its opposite one, and if any difference, to set it down; these differences I expected to find somewhere $= 0'' \, 0$, viz. in the diameter passing through the true centre of the circle, and the centre of the pivot, round which the machine revolved, and at right angles to this they would be at a maximum. The greatest quantity of this eccentricity, and the place where it lay, being ascertained, it would be easy to determine what it was in any other place; for this eccentricity, in any given part of the circle, would be as the co-sine of the distance from that point where it was at a maximum; and if, on this principle, a table was constructed, giving the eccentricity at every degree round the circle, the numbers in this table might be compared with the actual observation of the eccentricity, by the microscopes all round the circle, and if the quantity in the table did not every where agree with that found by experiment, the difference would be the actual error of that individual division; and in this manner the whole might be examined, every error detected, and a memorandum made of it. With this intention, and a full expectation of finding the eccentricity = 8″ or 10″, I set to work to examine the circle all round, having previously determined the diameter of the points in the arch to be about 21″, and the thickness of the wire = 12″. It is true, the points were not all exactly of the same size, nor could it be expected, but in general it might be concluded, that when the wire equally bisects a point, the segment on each side of the wire is about = 4″: so that an error of 1″ in the bisection can never be committed, with a tolerable light and reasonable care, the microscope magnifying 16 or 18 times. I placed the moveable wire of the east microscope so as to bisect the division 360°, and then, by repeated trials, made the moveable wire of the west microscope bisect the opposite point of 180°; in which, taking a mean of three or four observations, I could not err more than a few tenths of a second. The index of the micrometer screw being then carefully adjusted to zero (*o*) of the divisions of the head, I made every 10th degree of the circle pass under the micrometer wire of the east microscope, which wire might now be considered as fixed, and then noted whether the opposite division was under the moveable wire of the west microscope; if not, I wrote down the difference, after three or four times reading off. The result of these experiments may be seen in the adjoined table. Where

The 1st column shews the point, or division, that was brought under the wire of the east microscope.

The 2d column shews the want of coincidence, or how much the opposite point disagreed with the wire in the opposite microscope, at each reading off.

The 3d column gives the mean difference, and is = double the error of the centre + the sum of the errors of the two divisions.

The 4th column shews the difference of the mean reading off from the extremes, and may be considered as the greatest actual error in reading off these observations.

The 5th column contains the numbers in the 3d column corrected, by subducting 0″.9, a quantity which, it was found, upon taking the mean of all the numbers in the 3d column, the opposite or west micrometer was too forward upon the circle, viz. did not make a perfect diameter, but exceeded it in the order of the degrees by 0″.9. It will be observed that this quantity, from the three first series of observations of the points 360, and 180°, seemed to amount to 0″.6; this difference is inconsiderable. The numbers then in the 5th column, thus corrected, will express the true difference between the opposite divisions, if the wires in the microscopes had described a true diameter.

The 6th column gives the half of the difference just mentioned, and is = the simple error of the divisions.

Table of the Divisions of the Equatorial Circle.

1	2	3	4	5	6
East micro-scope.	West mi-croscope, and difference.	Mean diffe-rence.	Diffe-rence of the mean from the extremes	Column 3d cor-rected.	Simple error.
° ′	°	″	″	″	″
360 0	180. ″ + 0,0 1,2 0,7 1,0 0,6	+ 0,7	0,5	− 0,2	− 0,1
360° 0′ repeated	+ 0,4 0,8 0,9	+ 0,7	0,2	− 0,2	− 0,1
360° 0′ repeated	+ 0,8 0,2 0,0 0,5	+ 0,4	0,4	− 0,5	− 0,25
mean of these three		+ 0,6			
° ′ 10 0	° 190. ″ + 1,5 2,6 2,7	+ 2,3	0,8	+ 1,4	+ 0,7
20 0	200. + 1,0 0,7 1,0	+ 0,9	0,2	0,0	0,0
30 0	210. + 3,1 4,1 4,0 3,9	+ 3,8	0,7	+ 2,9	+ 1,45
40 0	220. + 4,2 3,9 4,2	+ 4,1	0,2	+ 3,2	+ 1,6
[illegible] 0	230. + 1,6 1,5 1,5	+ 1,5	0,1	+ 0,6	+ 0,3
60 0	240. + 2,8 3,6 3,6	+ 3,4	0,6	+ 2,5	+ 1,25
70 0	250.* + 3,2 3,0 2,0	+ 2,7	0,7	+ 1,8	+ 0,9

1	2	3	4	5	6
East micro-scope.	West mi-croscope, and difference.	Mean diffe-rence.	Diffe-rence of the mean from the extremes	Column 3d cor-rected.	Simple error.
° ′	°	″	″	″	″
80 0	260.″ + 3,4 3,5 3,8	+ 3,6	0,2	+ 2,7	+ 1,35
90 0	270. + 0,8 0,8 1,7	+ 1,1	0,6	+ 0,2	+ 0,1
100 0	280. + 1,5 2,2 2,4	+ 2,0	0,5	+ 1,1	+ 0,55
110	290.* + 0,7 − 1,2 − 1,7	− 0,7	1,0	+ 1,6	+ 0,8
111	291. + 0,8 1,5 0,4	+ 0,9	0,6	+ 0,0	0,0
120	300. − 0,5 − 0,7 + 0,5	− 0,2	0,5	− 1,1	− 0,55
130	310. + 0,7 − 0,5 − 1,0	− 0,3	1,0	− 1,2	− 0,6
140	320. − 2,7 − 1,6 − 2,3	− 2,2	0,6	− 3,1	− 1,55
150*	330. − 0,4 + 0,7 + 1,0	+ 0,4	0,8	− 0,5	− 0,25
160	340. − 2,2 − 1,2 − 2,0	− 1,8	0,4	− 2,7	− 1,35
170	350. − 1,6 − 2,2 − 1,7	− 1,8	0,4	− 2,7	− 1,35
Mean of all		− 0,9	0,53		

Those with * affixed are doubtful or bad points.

From inspection of the preceding table of observations it will readily appear, that I was much mistaken in my expectation of an eccentricity of 8 or 10″, for that, in truth, there seemed to be no fixed cause of error; and that therefore the error of the centre had little to do in occasioning those differences in the opposite microscopes, which only once amounted to 4″, and this in fact was double the error of the centre added to the sum of the errors in the two opposite divisions, together with the error of twice reading off; and that the simple error never exceeded, and but once amounted to 1″,6. This being the case, I think it fair to conclude, that the eccentricity never amounted to any sensible or measurable quantity, viz. never exceeded 1″,* and that consequently all the variety we see, in the west or opposite microscope, arose from the error of the divisions, inequality of the points, imperfection in reading off, or a little play in the screw of the micrometer. But, as all these together never but once amounted to 2″, I think it may fairly be presumed that *that* is the *greatest* error that will arise, in any observation made with this circle, when only one microscope is used, and that probably only half that error will take place.

* How extremely small a quantity this is, may be seen by considering that, on a radius of two feet, an arc

of 10′ amounts only to	- - - -	0,0698 inch.
of 1′ ————	- - - -	0,0070
of 1″ to (= in round numbers to about $\frac{1}{8600}$ inch.)		0,000116

Viz. to about 8 times less than the *minimum visibile* to the naked eye. This I reckon, with my own eye, at 8½ inches distance, is about $\frac{1}{1000}$ inch; but then it must be considered that the microscopes magnify 16 times, and will therefore render a space visible that is $\frac{1}{16000}$ inch, or about = 0″½, which we shall soon find to be the fact.

After this examination of the equatorial circle at every 10°, I did not think it necessary to proceed in the examination of each degree, and still less of every 10′, as I had intended. I therefore quitted this, and went to the declination circle, which underwent the same trial. The results of which will be seen in the adjoined table; the divided arch of the declination circle being turned towards the east.

Table of Observations of the Divisions of the Declination Circle.

1st microscope next the eye.	Farthest microscope next the object glass.	Mean difference.	Difference of the mean from extreme.	Simple error.
Divⁿ from	S. pole.	"	"	"
10° 0' " +0,2 −0,2 −0,3 +0,3 Mean 0,0	10° 0' " −0,3 −1,0 +0,7 −0,3	−0,1	−0,9	−0,05
20 0	20 0 −0,1 −0,5 −1,2	−0,6	0,6	−0,3
30	30 0 −0,5 −1,5 −0,7	−0,9	0,6	−0,45
40	40 0 −1,0 −0,9 −2,0	−1,3	0,7	−0,65
50*	50 0 −2,6 −3,7 −2,8	−3,0	0,7	−1,5
52° 10'	52° 10' +4,4 +5,2 +4,5	+4,7	0,5	+2,35
60*	60. −1,5 −1,0 +0,4	−0,7	0,8	−0,35
70	70. −1,4 −2,0 −2,6	−2,0	0,6	−1,0
80*	80. +2,4 +3,1 +2,6	+2,7	0,4	+1,35

1st microscope next the eye.	Farthest microscope next the object glass.	Mean difference.	Difference of the mean from extreme.	Simple error.
90° 0'	90° 0' " −1,0 −0,2 −0,8	−0,7	0,5	−0,35
100	100 +1,9 +0,4 +0,5 +1,0	+0,9	1,0	+0,45
110	110 +0,5 +0,6 −0,2	+0,3	0,5	+0,15
120	120 −0,5 +0,7 +0,4	+0,2	0,7	+0,1
130	130 −0,9 −0,6 +0,3	−0,4	0,7	−0,2
140*	140 +0,2 −0,1 +0,7	+0,3	0,4	+0,15
150*	150 −0,4 −0,3 −0,5	−0,4	0,1	−0,2
160	160 −0,1 +1,2 +0,2 +0,7	+0,7	0,6	+0,35
170	170 +1,2 +0,4 +0,3	+0,6	0,6	+0,3
180	180 +0,4 +0,6 +0,4	+0,5	0,1	+0,25
Mean	of all	+0,04	0,57	

* Denotes a doubtful or bad point.

From the preceding table of observations of the declination circle it appears, that these divisions were very little, if any, inferior in accuracy to those on the equatorial circle. And, finally it appears, that the errors, and probability of error, were as follows; viz. in the equatorial circle, from 22 corresponding observations of opposite divisions,

Obs^n.			
In 0,	the error amounted to	2″	therefore the probability against this error was
5.	- - about	$1\frac{1}{2}$	- - = $3\frac{1}{2}$ to 1
8.	- - -	1	- - = 2 to 1
14.	- - about	$0\frac{1}{2}$	- - = 1 to 2

And in the declination circle, from 19 corresponding observations of opposite divisions,

Obs^n.			therefore probability against this error,
In 1,	the error amounted to about	2″	= 18 to 1
3.	- - -	$1\frac{1}{2}$	= 5 to 1
4.	- - -	1	= 4 to 1
8.	- - -	$0\frac{1}{2}$	= 3 to 2

Therefore from 41 double observations on both circles,

Obs^n.			
In 1,	the error amounted to	2″	viz. = 40 to 1
8.	- - -	$1\frac{1}{2}$	= 4 to 1
12.	- - -	1	= $2\frac{1}{2}$ to 1
22.	- - -	$0\frac{1}{2}$	= 1 to 1

We may conclude, therefore, that in both circles no error of more than 2″ need be apprehended from the centre, and from the divisions taken together; and that in general it will probably not exceed 1″, on condition that the micrometer screw head is read off thrice, which in some observations may be done, if necessary. Lastly, from taking a mean of all the numbers in the 4th column, it seems the probable error, in reading off the divisions, is only 0″,58 in the equatorial circle, and 0″,57 in the declination circle. This source of error may therefore be put at 0″$\frac{1}{2}$; so that if one quadrant only of the circle should be made use of, viz. only one micrometer, and that only once read off, it is probable that no error of more than 1″$\frac{1}{2}$ will be committed. I mention this, because it will sometimes happen that only one such observation can be made; but where sufficient leisure will allow the reading off both the microscopes, this small error of 1″$\frac{1}{2}$ will probably be halved; and if the declination circle be turned half round, and the observation repeated, in the same manner, upon the two remaining quadrants of the circle, as is done (we shall presently see) when the line of collimation is examined, this error will probably be quartered, or reduced to less than 0″$\frac{1}{2}$.

After the very rigorous examination the divisions of these two circles have now undergone, and from the general knowledge that I have had opportunities to obtain of the state of practical astronomy in different countries; and when I consider that the celebrated artist, the late Mr. John Bird, seems to have admitted a probable discrepancy in the divisions of his 8 feet quadrants, amounting to * 3″, I think I am entitled to believe that the accuracy of these divisions under consideration is hardly to be equalled, and still less to be excelled, by that of any astronomical instrument in Europe; and, from the unexampled diligence and care, with which the skilful artist Mr. Matthew Berge, workman to Mr. Rams-

* See Mr. Bird's Method of constructing Mural Quadrants. London, 1768.

DEN, has executed them, I feel myself bound to bear this testimony to his merit.

(22.) It remains that I now say something of the power of the telescope; for it is to little purpose that the divisions be accurate, or the levels sensible, unless the force of the telescope be such as to correspond with the sensibility of the one, and the accuracy of the other. The object glass is a well corrected double achromatic, whose joint focus is 65 inches, with an aperture of 4,2 inches. The telescope is furnished with two sets of eye glasses, one single, the other double; of these latter there are 6, of different magnifying powers, from 60 to 360 times; of the former there are 5, with powers from 150 to 550. To these may be added a prism eye tube, with a power of about 100, for objects near the zenith, or the pole, and similar to the one described by General ROY; (see Philosophical Transactions, Vol. LXXX. p. 155) also a tube with a divided eye glass micrometer; (see Philosophical Transactions, Vol. LXIX.) it has a power of 80, but the images are not distinct, or equally bright, and the extent of the scale is so small, not more than 10′, that it is, in truth, but of little use. The double eye tubes are composed of two eye glasses, to enlarge the field and render it more agreeable, both placed on the hither side of the cross wires, so that they may at any time be changed, without deranging the wires. The lowest of the compound eye tubes, with a power of about 60, is what is generally used for transits and polar distances.* For telescopical observations

* If, as has been generally imagined, an angle of 1′ is about the smallest that is visible to the naked eye, (SMITH's Optics, § 97) with a power of 60 times 1″ will become visible; and, in that case, the power of this telescope will correspond with the levels, and the divisions, as was required above.

of the planets, higher powers may be put on; and of these, that of 400 seems to be near the maximum that this glass will bear; with 500 the image is not so well defined; with 200 or 300, it is beautifully distinct and bright; but this inquiry demands more experiments than I have hitherto made, having been able to procure these high powers only within a few weeks.

(23.) Having now given, as I apprehend, a very satisfactory idea of the accuracy of the parts of this instrument, I shall proceed to the method of adjusting them. This machine, not being capable of having its polar axis laid horizontal, its adjustments, in some respects, will be different from those of small instruments of the same name. (See the methods proposed by Mr. RAMSDEN, in his Description, and by Mr. VINCE, in his Practical Astronomy.) It may be proper to premise, that the principal points required are, 1st. to adjust the level P, *k*, parallel to the declination axis U, V. 2dly, to adjust this axis at right angles to the line of collimation of the telescope; and, 3dly, to make this axis at right angles to the polar axis.

Probably others may be devised, but the mode I have adopted is as follows.*

The polar axis is placed nearly in the meridian, by means of a meridian mark, previously verified, and elevated pretty nearly to the latitude of the place. This is to be done more accurately afterwards, by the sliding plates, and screws, at the bottom of the polar axis.

* Whoever is desirous of seeing some very ingenious disquisitions of the errors and adjustment of this instrument, will do well to consult the Abbé BOSCOVICH'S *Opera pertinentia ad Astronomiam et Opticam*, quarto, Bassani, 1785. Tomi 4ti Opusculum 14tum.

The axis of the declination circle is then brought nearly horizontal, by its proper level, viz. is turned round about the polar axis, till the bubble of the level stands true between the indexes; the instrument is then turned half round about the polar axis, = 180°, shewn by the microscope W. If the bubble then stand true, it requires no correction, but if it do not, correct half the error, by moving the equatorial circle by its handle *t*, and the other half by the capstan screw *a*; (Tab. XIV. fig. 1) then turn the instrument back again to its first position, and see if the level stand true; if not, repeat this operation till it does, correcting one half of the error by the equatorial handle, and the other half by the screw *a*. The declination axis will then be parallel to the level, and both of them to the horizon. It must be remarked, that in this operation it will be necessary to move the declination circle round its own axis a little, in order to bring the same side of the level uppermost; but this in no degree affects the result, for the imaginary line, round which this axis revolves, is what is meant all along by the axis, and is the line to which the parallelism of the level is referred.

The declination axis remaining in an horizontal position, with the level above the axis, as in Tab. IX. turn the declination circle 180°, viz. till the level become below the axis; then, by means of the pinion *b*, restore the tube of the level to an upright position, and see if the bubble stand true; if not, correct $\frac{1}{2}$ the error by the screw *c*, and the other $\frac{1}{2}$ by *a*. Now turn the declination circle 90° each way from its last situation, and repeat the examination of the bubble, and correct, as before, $\frac{1}{2}$ by the screw *b*, at right angles to *c*, and $\frac{1}{2}$ by the screw *i*; and if, after all these corrections, in every part of an entire revolution of the declination circle round its axis, and of the level round its axis, the bubble stand true, it follows, that the axis of the declination circle, and of the level, are in every direction parallel to each other, both of them to the tangent of curvature in the middle of the level, and all three to the horizon. This adjustment is therefore complete.

(24.) It remains to be seen, whether the line of sight of the telescope is at right angles to the declination axis, and this latter to the polar axis.

Take the error of the collimation of the telescope in right ascension, by a star in the equator, viz. let the transit of a star in the equator over the assumed meridian be observed, with the declination circle turned towards the east, and also towards the west. If there be any difference in these observations, it will denote double the error of collimation in right ascension, and half of it will be the deviation of the line of sight from a line at right angles to the axis of the declination circle; and is correspondent to a similar adjustment of a transit instrument. The amount of this error being thus ascertained, let it be corrected by the screws, at the eye end of the telescope, that move the wires to the east and west. The declination axis, by means of its level, being restored to an horizontal position, let the centre wire of the telescope (by which is always understood the line of collimation) be brought to bisect the meridian mark, by means of the sliding plate and adjusting screw below the polar axis, the telescope will then become a complete transit instrument; for, by the *first* operation, the declination axis is made parallel to the level and its axis, and both to the horizon; by the *second*, the line of sight is put at right angles to this axis; and *thirdly*, it is adjusted to the meridian.

Now, let the error of collimation in right ascension, in the same manner, be observed with any star out of the equator, by a circumpolar star, (the nearer the pole the better) suppose the pole star. If any difference should be noticed in its passage, with the circle east or west, halve that difference,* and it will be equal to the angle that the plane of the declination circle makes with the polar axis, if the observed star were actually in the pole; if not, divide it by the sine of its declination, and the true angle of the plane of this circle (or of the line of collimation) with the polar axis, will be had. Again, if this operation be repeated with any other stars, and the error so found be divided by the sine of their declination, the error of the plane of the declination circle at the pole, viz. its greatest error, or angle with the polar axis, will be had. And note, if these observations are made with stars on each side of the equator, these quantities will be had in opposite directions. Finally, the same may be done by two land objects, one to the north, and the other to the south; the north and south meridian marks, for instance, proper consideration being had to their declination; by this means the error will be thrown in contrary senses, or doubled, and, from a variety of such results, a very correct *mean* quantity may ultimately be

* By difference is here meant, the difference taken in minutes and seconds of a great circle passing through the star, and which can only be directly measured by a micrometer; but if, as is most convenient, this quantity should be observed by time, or by the divisions on the equatorial circle, (15 and 16) this quantity must be diminished in the proportion of the radius to the sine of the polar distance, viz. multiplied by the co-sine of the declination; hence it is, that this method is capable of great precision.

deduced; and when found, must be corrected by the screws* at one end of the declination axis. I have been rather diffuse in the account of this adjustment, because it is one of the most important in the whole instrument, and does not readily present itself.

It has now been seen that, 1st, the level and its axis are parallel to the axis of the declination circle. 2dly, the line of sight at right angles to this axis, and parallel to the polar axis; and consequently the declination axis at right angles to the polar axis. 3dly, the polar axis parallel to that of the earth. These are the chief requisites in the adjustment of this instrument. Those that remain are secondary, and I shall take them in the following order. 1st. The adjustment of the cross wires to the focus of the telescope. 2dly. The hanging level. 3dly. The line of collimation, north and south, as well as east and westward. 4thly. The index wires in the microscopes. 5thly. The refraction apparatus. And, 6thly, the power and scale of the microscopes.

(25.) First, the cross wires. Let the eye tube be adjusted to distinct vision for parallel rays by some distant object, such as Jupiter, Saturn, or Venus, by daylight; that done, observe, while one limb of either of these planets appears running along the equatorial wire, whether any motion of the eye, upwards or downwards before the eye glass, alters the relative place of the image and the wire; if a motion of the eye upwards moves the planet in the *same* direction, the wires are too near the eye glass, and must be pushed in; and *vice versâ*, till the image become fixed upon the wire, what-

* The heads of these screws being divided into 10 parts, and the value of each known, any given correction is easily applied.

ever be the motion of the eye. When this point is obtained, the eye stop with its wires, must be there fixed, for that is their true place; viz. the correct focal point of the object glass; and whatever indistinctness may be found, from the diversity of eyes of different observers, must be corrected by the motion of the eye glass only. Another point to be secured is the permanency, as far as may be, in the position of the object glass; for if this be not correctly centered, which is very rarely the case, and indeed never to be expected, that is, if its axis be not concentric with the axis of the cell, in which it is fixed, any motion of this latter, by screwing or unscrewing it, may not only change the place of the focus, to which the wires are adjusted, but will necessarily move the line of collimation, both in right ascension and declination.* To obviate this, therefore, two corresponding marks should be made, with a graver, both upon the cell, into which the glass is burnished, and also upon the tube of the telescope, into which the cell is screwed, or otherwise inserted, that in case the object glass should ever be taken out to clean it, &c. it may be restored very nearly, if not exactly, to its former position.

The eye glass, object glass, and wires, being thus settled in their respective places to each other, it will not be an improper time to measure the interval between the wires, which cannot be too accurately done, being of such constant use; this may be either, 1st, by observing the passage of a star in the equator, and making proper allowances for the rate of the

* By moving my object glass an entire revolution in its screw, the line of collimation appears to move through a little circle of 50″ in diameter, so that the eccentricity, in this instance, appears to have been about $\frac{1}{100}$ inch.

clock, or by a star out of the equator, and making proper allowance for the declination, in the proportion of the radius to the co-sine: or, 2dly, by means of the equatorial circle and a fixed land object; and here the quantity must be diminished in the same ratio of the radius to the sine of the polar distance. I have made use of both methods, as a confirmation of each other, and find the interval, which is equal in the three wires of my telescope, to be 7′ 34″,5 = 30″,3 sidereal time; and these three wires divide the diameter of the field very nearly into four equal parts.

(26.) Second and third adjustment; the hanging level. By means of its proper handle *u*, move the declination circle about its axis, till the bubble of the hanging level *l*, *m*, rests true between the indexes, there fix it by the clamp *w*, reverse the level, by taking it out of its pivots, and turning it end for end; if the bubble now stand true, the level is adjusted; if not, correct $\frac{1}{2}$ the error by the declination handle, and the other $\frac{1}{2}$ by the small screw at the bottom of the level; then reverse the level, and repeat this operation till it does. The level, or rather a tangent to its curvature at its middle, will be parallel to the axis, on which it swings; and both will be horizontal. At this time look through the telescope, and see what land object is covered by the horizontal wire; now invert the telescope, by turning it 180° round the declination axis, and 180° round the polar, and bringing the level true, it will then point to nearly the same place; and if exactly the same object as before be now covered by the horizontal wire, the axis of the level is adjusted parallel to the line of collimation, in a vertical direction; if not, correct half the error by the little capstan screw at the bottom of the cock, or arm, that supports one end of the axis of the level, and the other half by the declination handle; invert the telescope, and repeat the operation till the same object is covered in both positions, and the level is found true; then will the level and its axis be parallel to the line of collimation, and the object covered by the wire may be concluded to be in the horizon.

(27.) Fourth. The index wires of the microscopes. The line of collimation, with respect to east and west, has been already adjusted as above (sect. 24.) Let then the declination axis, by its level, be restored to an horizontal position; at this time adjust the index wires in the two equatorial microscopes W X, to bisect the two opposite divisions 360 and 180°, then will these wires be rectified to their proper place. That being done, bring 90°, or the division that represents the equator on the declination circle, under its respective microscope, and turn the whole instrument one quarter round on the polar axis, viz. till 90° on the equatorial circle be bisected by the micrometer; and if, at this time, the bubble of the hanging level appear true, the index wire of the declination microscope is correct; if not, correct half the error by the declination handle *u*, and half by the little screws *b*, (Tab. XIV. fig. 2) at the side of the hanging level; then reverse the telescope, viz. turn it till 270° on the equatorial circle come under the micrometer wire, and if the level then rest true, the adjustment is complete; if not, repeat the operation, as before, till it does; then, by its proper screw, bring the index wire of the declination micrometers to bisect the points 90 and 90°. The indexes of both circles will be then adjusted, and the axis of the hanging level brought parallel to the line of collimation, with respect to east and west, as well as with respect to north and south. Note, this parallelism of the axis of the level, to the line of collimation in a direction east and west, does not appear to be a very important rectification, but on some occasions may have its use.

(28.) Fifth. The refraction piece. After what has been done, this apparatus will be easily adjusted. Bring the telescope, by means of its two levels P *k*, and *l m*, to point to the horizon, and in the meridian; then, by the two pinions *f*, *h*, (Tab. X. fig. 2) of the refraction piece, bring *its* two levels *e* and *g* to rest true; move the nonius *d*, of the little semicircle of the horary and vertical angles *a*, *b*, *c*, to the middle of the divisions, or 0° 0′, and also that of the little quadrant of altitude *l*, *k*, to 0° 0′, and this part is adjusted.

(29.) Sixth. The microscopes. The magnifying power and scale of the microscopes is all that remains to be considered.

The magnifying power of a compound microscope, as is well known, (see SMITH's Optics, § 127) depends on the proportion between the distance of the object, and of its image, from the object glass, together with the proportion between the focus of the eye glass, and ordinary focus of the eye, looking at a small object (suppose of $\frac{1}{50}$ or $\frac{1}{100}$ inch). These two ratios compounded give the power of the microscope. The former is called magnifying by distance, and is a material part in the construction of these microscopes; the scale of the micrometer being regulated by this part of the magnifying power. For example, let the distance of the object from the glass be = 1, and the distance of its image = 4, its power will be 4; and consequently the scale of the micrometer, or motion of its screw, to answer to 10′, (suppose) must be

4 times as great as the space occupied by 10′ on the limb of the circle; and if the radius of the circle be 2 feet, an arc of 10′ will be equal to 0,07 inch nearly, on the limb; and = 0,28 inch on the scale, viz. = to the same arc on a circle of 8 feet radius; and if each revolution of the micrometer screw be intended to describe 1′, the screw must contain about 35 threads in an inch. But as it would be difficult to adjust the screw *exactly* to the scale, the advantage of the construction of these micrometers is, the scale may at any time be adjusted to the screw; for let the interval between any two nearest divisions, = 10′, on the limb, be measured by the screw, and suppose, instead of being = 10′ or 600″, it appears only = 570″; it is evident, that the scale is bigger than it should be, or, which is the same thing, that the image is less by $\frac{30}{600}$ or $\frac{1}{20}$. In this case increase the distance between the micrometer wires, and the object glass, = $\frac{1}{20}$, by unscrewing or drawing out the tube, that carries the micrometer and eye glasses, and the scale is adjusted. It will at the same time, however, be necessary to re-adjust the object glass of the microscope to distinct vision, by the screw of the cell, that contains it, until the image and the wires have no relative change of place by any motion of the eye. This will again occasion some small alteration in the scale, and must be corrected by repeated trials, and the scale adapted to the divisions on the arc; and if the moveable wire of the microscope be now brought to coincide exactly with the fixed one, and the moveable index (with the mark ⊥) brought to zero (o) on the screw head, the micrometer is completely adjusted. This having been done with all the microscopes, and the opposite ones being made to agree, each with the other, in such

manner, that the fixed wires may become a correct diameter, I believe the whole instrument will have been completely adjusted.

(30.) Before I conclude this account, I must beg leave to trouble the reader's patience a few minutes more, in order to give a general idea of the manner of making the ordinary observations of right ascension and declination, with this instrument.

Let the telescope be adjusted towards the meridian mark, in such manner that the centre wire may exactly bisect it; then note if the index wire of the equatorial micrometers bisect the points of 360° and 180°. If it does, the instrument is prepared for observing a transit; if not, and the difference be considerable, it must be corrected by moving the polar axis, by its adjusting screw. But as this quantity will seldom exceed 8″ or 10″, it will be more convenient to note this quantity in the journal, and allow for it afterwards in reducing the observations.* As this quantity will from time to

* This may be done by the following rule.

$Æ \times \text{sine } P \times \frac{\text{sine zenith dist.}}{\text{sine polar dist.}} = x$ the correction + or — according as the telescope points to the east or west of the meridian.

Where Æ is = the error on the equatorial circle,

And P = the angle that the polar axis makes with a ray from the meridian mark.

And if there should appear any error in the horizontal position of the axis of the declination circle at this time, by the level not standing true, that error may be corrected by this theorem.

$D \times \frac{\text{sine alt.}}{\text{sine polar dist.}} = x$ the correction.

Where D is = angle of depression of one end of this axis below the horizon.

And by means of the above theorems, a table may be calculated that will give these corrections always, by inspection; such a table I have computed for my own use, but time be variable, from a variety of causes, such as the possible settling of the walls of the building; the partial or irregular expansion of the instrument, from the sun-beams accidentally falling upon it; from the effect of a fire in the room, or the heat of a person's body in cold weather; from the sun's heat upon the meridian mark, eastward in the morning, and westward in the afternoon; from the same effect upon the observatory; and lastly, possibly from a *lateral* refraction of the ray coming from the meridian mark, from irregular vapours floating near the surface of the earth. From some, or all of these causes, this quantity of error will be found to be exactly the same hardly for two days together; but I have never yet known it to exceed 13″ of a degree, = 0″,7 in time, during a period of more than a year, and very rarely above 6″ or 7″, sometimes on one side, and sometimes on the other. When this error is known, and set down, move the instrument about the polar axis by its handle *t*, till the divisions 360 and 180° are bisected by the equatorial wire; this done, move the declination circle by its handle *u*, till the hanging level stands true; and note the division on the declination circle cut by the micrometer wire, for that is the horizontal point, from whence the altitudes are to be reckoned. If the order of the divisions be such as to shew declinations, this division will be the angle of the co-altitude of the polar axis; but if the divisions be such as in my instrument, it will be equal to the altitude of the polar axis, which *should* be equal to the latitude of the place. But

as it is suited only to one latitude it is not given here. Tables, something similar to this, may be seen in Mr. LUDLAM's Astronomical Observations, Cambridge, 1769; and also in the *Connoissance des Temps, pour* 1792, p. 251.

as this will seldom happen, from the same reasons as have already been mentioned, I prefer taking a memorandum of this quantity also, and allowing for it; by which means I see its alteration, from time to time, compared with the weather, know better what I am about, than if I attempted to correct it, and save a great loss of time. From hence it must be seen that nothing is depended on, but that the instrument keeps its place during the four or five minutes that the observer is occupied in making the observation.

(31.) The instrument is now prepared for an observation over the meridian, and also of the polar distance; at which moment, if the weather should be cloudy, the observation incomplete, or unsatisfactory, it may be repeated as many times afterwards as is thought proper, taking notice of the distance from the meridian, shewn in the equatorial microscopes, and making allowance, in the reduction, for the motion of the sun, or planet, during the interval; for I esteem an observation made within 10 or 15° of the meridian, nearly equivalent to a meridian observation. But, if an observation should be made out of the meridian, the altitude and angle of the horary and vertical circles must be taken, by the refraction apparatus; and with these arguments, the refraction and parallax, in north polar distance, and in right ascension, may be found by inspection, in the tables that follow this account, and consequently every observation readily reduced to the meridian.

(32.) I shall close this long history with an account of the probable accuracy of the observations made with this instrument, viz. of the amount of the probable errors, derived from an experience of more than twelve months. And first, with respect to those of right ascension. It must be readily seen, that the amount of these errors will be pretty nearly the same as those of any other transit instrument, whose magnifying power, and length of axis, are the same. However, from actual trial, I find that the passage of a star, near the equator, over any one wire in the field of the telescope, may be determined to within $\frac{1}{3}$ of a beat of the regulator, in strictness to about $3'',7$, and, from a mean of the three wires, to within $1'',25$ of a degree, $= \frac{1}{12}$ of a second of time; that is, if the wind be still, the weather favourable, and reasonable care be taken. And, from a series of observations of the sun's diameter throughout the year, it appears, the error in ordinary observations may be expected to lie within $3''$; that it is 17 to 1 that this error does not amount to $5'', = \frac{1}{3}$ of a second of time; and this includes some of the worst weather in which observations are likely to be made. I shall therefore say, that

The probability of error of an observation of a transit over the meridian, under tolerably favourable circumstances, from a mean of 3 wires, viz. in estimating the beat of the regulator, is about - - - $= 2,0''$

Add to this the error of setting the instrument to the meridian mark - - - - $= 1,0$

Add, also, the error in reading off the equatorial microscope - - - - - $= 0,5$

Total error of an observation *in* the meridian becomes - - - - - $= 3,5$

To this add the error of the divisions, and of the centre (at most) - - - - $= 1,0$

Also the second reading off of the microscope - = 0,5″

And the total error of an observation of a transit *out* of the meridian, will be - - - = 5,0

The same, from actual experiment, in 13 trials, within 15°, on each side of the meridian (in February, 1792) - - - - - = 7,5

That is, that an observation made out of the meridian, will give the transit over the meridian true to $\frac{1}{2}$ a second of time.

The error in the observation of a polar distance may be put as follows.

Error of the eye, in estimating the coincidence of the wire in the telescope with the object, the power being 60 times - - - - = 1,″

Error of the divisions, and of the centre, in taking the horizontal point in the circle - - - = 1,

Error in reading this division off by the microscope - - - - - = 0,5

Error of the level, in ordinary observations - = 2,

Error of the divisions, and centre, a second time, viz. in taking the angle of the polar distance - = 1,

Error in reading off this division by the microscope = 0,5

Sum of all these errors - - - = 6,0

Ditto, by actual observation of the line of collimation, the circle being turned east and west, from various experiments, appears to be - - 7,5″

Lastly, if care be taken in the observation, and the sun not suffered to shine on the instrument, only during the moment of observation, I think no error in the polar distance need be apprehended exceeding - ″ 7,

And in the meridian passage, none exceeding - 3,5

So that it appears, that the right ascension will be observed with twice the precision of the polar distance.

And here we must not omit to take notice, that of all the above mentioned causes of error, one only, viz. the error of the divisions in taking the polar distances, appears to be fixed, so that, by repeating the observation, the truth may be approached to any given degree of accuracy. I have thought proper to make these remarks on the errors of the instrument, that if health and leisure should enable me hereafter to offer to the Royal Society the result of any astronomical observations with it, it may be known to what claim to precision they are entitled.

In describing the instrument, the following references in Tab. IX. were omitted in the text.

ff, gg, hh, ii, are the eight conical radii to the declination circle, described page 78.

11, 12, are two supporters to the clamp and endless screw, page 80.

36, is a pair of steps, for the convenience of the observer.

Explication and Use of the following Tables.

The three first Tables are particularly calculated for the use of the large equatorial instrument, for the purpose of clearing observations with it from the effects of refraction and parallax. The four last are adapted more peculiarly to the small or portable equatorial, such as has been noticed in sect. 10, and which I thought might be acceptable to such persons as have the good fortune to possess one of these instruments. Of these Tables in their order.

Table I. gives the correction of the refraction in north polar distance, by entering it with the altitude at the top, and the angle of the horary and vertical circles on the left hand side; and in the common point of meeting is found a quantity, in seconds and decimals, that is to be added to the *apparent* polar distance, to give the *true*; this correction is always additive. But if the same table be entered with the angle of the horary and vertical circles, in the right hand column, it will give the refraction in right ascension, by multiplying the quantity here found by the secant of the declination, to be found in Table IV.

Table II. gives the effect of the sun's parallax in right ascension and north polar distance, and is to be entered with the same arguments as Table I.; and the parallax in right ascension is to be multiplied by the secant of the declination, as before; the sun's horizontal parallax being assumed $=8'',6$.

Table III. is a similar Table, only calculated to an horizontal parallax of 10'': so that whatever be the parallax of the sun or planets, this correction may readily be found, almost by inspection.

Table IV. is the natural secants to each degree, extracted from SHERWIN's Tables; being of such constant use in these computations, I have placed it with these tables of refraction.

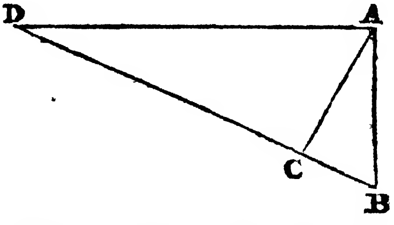
Table V. gives the correction of the time; viz. of the sun or star's distance from the meridian, in an observation with a portable equatorial, not previously adjusted to the meridian; this quantity is to be multiplied also by the secant of the declination.

Table VI. as its title imports, gives the correction of the meridian line in minutes and decimals, which was thought near enough for a portable instrument; the quantity here found is to be multiplied by the secant of the altitude.

Table VII. similar to Table I. only reduced to seconds of time, gives the refraction in right ascension, suited to the usual mode of dividing the small instrument; viz. into civil hours and minutes.

Table VIII. similar also to Table I. gives the refraction in declination. The arguments are the same in all the Tables; viz. the altitude, and the angle of the horary and vertical circles; which appeared to me the only means of making the Tables universal, and adapted to all latitudes.*

Their foundation is this:



Let AB be a portion of a vertical circle = the refraction in altitude; DA a parallel to the horizon; and DB a paral-

* A table of refraction in right ascension and declination, for the latitude of Paris *only*, may be met with in the *Connoissance des Temps, pour* 1791. What I have given in the following account, viz. Tab. V. VI. VII. and VIII. I calculated for my own use, as early as the year 1774.

lel to the equator: then AC will be a portion of an horary circle = the refraction in declination, found by Table I. and Table VIII. ∠ CAB, the angle of the horary and vertical circle; CB, the refraction in right ascension, found by Table I. and Table VII. DA, the correction of the meridian, found by Table VI.; and DB, the correction of the time, found by Table V.; and, as AB will hardly ever be found to exceed 30′, these triangles have been considered all as plane; making due allowance, in the proportion of the sine to the radius, for the distances of the arcs DA, BC, and DB from their respective poles, which has been noticed at the foot of each Table.* The refraction in altitude having been taken from Professor MAYER'S Tables, London edition, 1770, which is calculated for a density of the air, expressed by 29,6 inches of the barometer, and 50° of FAHRENHEIT'S thermometer; and, for any other heights of the barometer and thermometer, may be corrected in the usual way; making an allowance for each degree of FAHRENHEIT'S thermometer, above or below 50°, of $\frac{1}{429.5}$. This correction has been deduced from the result of a great many observations that I made some years since with the manometer, described in the Philosophical Transactions for the year 1777, Vol. LXVII. p. 564. The equation that astronomers have generally been used to adopt, from Dr. BRADLEY'S Observations, is $\frac{1}{400}$ for each degree of the thermometer; but, I think, erroneously.

* I have proposed multiplying by the secant, instead of dividing by the co-sine, as being the readier operation, and which comes to the same thing.

No I. Table of the Effect of Refraction in North Polar Distance.

This correction is always +.

Angle of the vertical with the horary circle.	DEGREES OF ALTITUDE.															Angle of the vertical with the horary circle.
	2°	4°	6°	8°	10°	12°	14°	16°	18°	20°	22°	24°	26°	28°	30°	
°	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	
0	18 1	11 42	8 25	6 30	5 15,6	4 24,0	3 46,4	3 17,5	2 54,7	2 36,3	2 20,9	2 8,0	1 57,2	1 47,6	1 39,0	90
2	18 0	11 42	8 24	6 30	5 15,3	4 23,8	3 46,3	3 17,4	2 54,6	2 36,2	2 20,8	2 7,9	1 57,1	1 47,5	1 38,9	88
4	17 58	11 40	8 24	6 29	5 14,7	4 23,4	3 45,9	3 17,0	2 54,3	2 35,9	2 20,6	2 7,7	1 56,9	1 47,3	1 38,8	86
6	17 55	11 37	8 22	6 28	5 13,8	4 22,5	3 45,2	3 16,4	2 53,7	2 35,4	2 20,1	2 7,3	1 56,6	1 47,0	1 38,5	84
8	17 50	11 35	8 20	6 26	5 12,6	4 21,4	3 44,2	3 15,6	2 53,0	2 34,8	2 19,5	2 6,8	1 56,1	1 46,6	1 38,0	82
10	17 44	11 31	8 17	6 24	5 10,8	4 20,0	3 43,0	3 14,5	2 52,0	2 33,9	2 18,8	2 6,1	1 55,4	1 46,0	1 37,5	80
12	17 37	11 27	8 14	6 21	5 8,7	4 18,2	3 41,4	3 12,8	2 50,9	2 32,9	2 17,8	2 5,2	1 54,6	1 45,2	1 36,8	78
14	17 29	11 21	8 10	6 18	5 6,3	4 16,2	3 39,7	3 11,6	2 49,5	2 31,7	2 16,7	2 4,2	1 53,7	1 44,4	1 36,1	76
16	17 19	11 15	8 5	6 15	5 3,3	4 13,7	3 37,6	3 9,8	2 47,9	2 30,2	2 15,4	2 3,0	1 52,7	1 43,4	1 35,2	74
18	17 8	11 7	8 0	6 11	5 0,3	4 11,1	3 35,3	3 7,8	2 46,1	2 28,7	2 14,0	2 1,7	1 51,5	1 42,3	1 34,1	72
20	16 56	11 0	7 54	6 7	4 56,7	4 8,1	3 32,8	3 5,6	2 44,1	2 26,9	2 12,4	2 0,3	1 50,1	1 41,1	1 33,0	70
22	16 41	10 51	7 46	6 1	4 53	4 4,8	3 29,9	3 3,2	2 42,0	2 24,9	2 10,6	1 58,7	1 48,7	1 39,7	1 31,8	68
24	16 28	10 41	7 41	5 56	4 48	4 1,2	3 26,9	3 0,5	2 39,6	2 22,8	2 8,7	1 56,9	1 47,1	1 38,3	1 30,4	66
26	16 12	10 31	7 34	5 50	4 44	3 57,3	3 23,5	2 57,6	2 37,0	2 20,5	2 6,6	1 55,0	1 45,4	1 36,7	1 29,0	64
28	15 55	10 20	7 26	5 44	4 39	3 53,1	3 20,0	2 54,5	2 34,2	2 18,0	2 4,4	1 53,0	1 43,5	1 35,8	1 27,4	62
30	15 36	10 8	7 17	5 38	4 33	3 48,6	3 16,1	2 51,1	2 31,3	2 15,1	2 2,0	1 50,8	1 41,5	1 33,1	1 25,7	60
32	15 17	9 55	7 8	5 31	4 28	3 43,9	3 12,0	2 47,6	2 28,1	2 12,6	1 59,5	1 48,5	1 39,4	1 31,2	1 24,0	58
34	14 56	9 42	6 58	5 23	4 22	3 38,9	3 7,8	2 43,8	2 24,8	2 9,6	1 56,8	1 46,1	1 37,2	1 29,1	1 22,1	56
36	14 34	9 28	6 48	5 16	4 15	3 33,6	3 3,2	2 39,9	2 21,3	2 6,5	1 54,0	1 43,6	1 34,9	1 27,0	1 20,1	54
38	14 12	9 13	6 38	5 7	4 9	3 28,0	2 58,4	2 35,6	2 17,6	2 3,2	1 51,0	1 40,9	1 32,4	1 24,7	1 18,0	52
40	13 48	8 58	6 27	4 59	4 2	3 22,2	2 53,4	2 31,3	2 13,7	1 59,8	1 47,9	1 38,0	1 29,8	1 22,3	1 15,8	50
42	13 23	8 41	6 15	4 50	3 55	3 16,1	2 48,2	2 26,7	2 9,7	1 56,1	1 44,7	1 35,1	1 27,1	1 19,9	1 13,5	48
44	12 58	8 25	6 3	4 40	3 47	3 9,9	2 42,9	2 22,1	2 5,6	1 52,4	1 41,4	1 32,1	1 24,4	1 17,4	1 11,2	46
46	12 31	8 7	5 51	4 31	3 39	3 3,4	2 37,3	2 17,1	2 1,4	1 48,6	1 37,9	1 28,9	1 21,5	1 14,7	1 8,8	44
48	12 3	7 49	5 38	4 21	3 31	2 56,6	2 31,5	2 12,1	1 56,9	1 44,6	1 34,3	1 25,6	1 18,5	1 12,0	1 6,2	42
50	11 35	7 31	5 24	4 11	3 23	2 49,7	2 25,6	2 6,9	1 52,3	1 40,5	1 30,6	1 22,3	1 15,4	1 9,1	1 3,6	40
52	11 6	7 12	5 11	4 0	3 14	2 42,5	2 19,3	2 1,6	1 47,5	1 36,6	1 26,8	1 18,8	1 12,2	1 6,2	1 1,0	38
54	10 35	6 52	4 57	3 49	3 6	2 35,2	2 13,8	1 56,6	1 42,7	1 31,9	1 22,9	1 15,2	1 9,0	1 3,2	58,2	36
56	10 4	6 33	4 42	3 38	2 57	2 27,6	2 6,6	1 50,5	1 37,7	1 27,4	1 18,8	1 11,6	1 5,6	1 0,1	55,4	34
58	9 33	6 12	4 27	3 27	2 47	2 19,9	2 0,0	1 44,7	1 32,6	1 22,9	1 14,7	1 7,8	1 2,2	57,0	52,5	32
60	9 0	5 51	4 12	3 15	2 38	2 12,0	1 53,2	1 38,7	1 27,3	1 18,1	1 10,4	1 4,0	58,6	53,8	49,5	30
62	8 27	5 30	3 57	3 3	2 28	2 3,9	1 46,3	1 32,7	1 22,2	1 13,3	1 6,2	1 0,1	55,0	50,5	46,5	28
64	7 54	5 8	3 41	2 51	2 18	1 55,7	1 39,3	1 26,6	1 16,6	1 8,5	1 1,8	56,1	51,4	47,1	43,4	26
66	7 19	4 46	3 25	2 39	2 8	1 47,4	1 32,1	1 20,3	1 11,1	1 3,5	57,3	52,1	47,7	43,7	40,3	24
68	6 45	4 23	3 9	2 26	1 58	1 38,9	1 24,9	1 14,0	1 5,5	58,5	52,8	47,9	43,9	40,3	37,1	22
70	6 10	4 0	2 52	2 13	1 48	1 30,3	1 17,5	1 7,6	59,8	53,5	48,2	43,8	40,1	36,7	33,9	20
72	5 34	3 37	2 36	2 1	1 37	1 21,6	1 9,9	1 1,1	54,0	48,3	43,6	39,6	36,3	33,2	30,6	18
74	4 58	3 13	2 19	1 47	1 27	1 12,8	1 2,4	54,5	48,1	43,1	38,9	35,3	32,6	29,6	27,3	16
76	4 22	2 50	2 2	1 34	1 16	1 3,9	0 54,8	47,8	42,1	37,8	34,1	31,0	28,4	26,0	24,0	14
78	3 45	2 26	1 45	1 21	1 5	0 54,9	47,1	41,1	36,1	32,5	29,3	26,6	24,3	22,4	20,6	12
80	3 8	2 2	1 27	1 7	0 55	45,8	39,3	34,3	30,2	27,1	24,4	22,2	20,3	18,6	17,2	10
82	2 30	1 38	1 10	0 54	0 44	36,7	31,6	27,5	24,3	21,7	19,6	17,8	16,3	14,9	13,8	8
84	1 53	1 13	0 53	0 41	0 33	27,6	23,5	20,7	18,4	16,3	14,7	13,4	12,2	11,4	10,3	6
86	1 15	0 49	0 35	0 27	0 22	18,4	15,8	13,7	12,3	10,9	9,8	8,9	8,2	7,5	6,9	4
88	0 38	0 25	0 17	0 13	0 11	9,3	7,9	6,9	6,2	5,5	4,9	4,5	4,1	3,8	3,5	2
90	0 0	0 0	0 0	0 0	0 0	0 0,0	0 0,0	0 0,0	0 0,0	0 0,0	0 0,0	0 0,0	0 0,0	0 0,0	0 0,0	0

Refraction in right ascension, × sec^t declination.

This correction is — on the east, and + on the west side of the meridian.

No I. Table of the Effect of Refraction in North Polar Distance, continued.

This correction is always +.

Angle of the vertical with the horary circle.	DEGREES OF ALTITUDE.															Angle of the vertical with the horary circle.
	32°	34°	36°	38°	40°	42°	44°	46°	48°	50°	52°	54°	56°	58°	60°	
°	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	″	″	″	″	″	″	″	″	″	°
0	1 31,5	1 24,7	1 18,7	1 13,2	1 8,2	1 3,6	59,3	55,3	51,6	48,1	44,9	41,7	38,7	35,8	33,1	90
2	1 31,4	1 24,6	1 18,7	1 13,2	1 8,2	1 3,6	59,3	55,3	51,6	48,1	44,9	41,7	38,7	35,8	33,1	88
4	1 31,3	1 24,5	1 18,5	1 13,2	1 8,0	1 3,4	59,2	55,2	51,5	48,0	44,8	41,6	38,6	35,7	33,0	86
6	1 31,0	1 24,2	1 18,3	1 12,8	1 7,8	1 3,3	59,0	55,0	51,3	47,8	44,7	41,5	38,5	35,6	32,9	84
8	1 30,6	1 23,9	1 17,9	1 12,5	1 7,5	1 3,0	58,7	54,8	51,1	47,6	44,5	41,3	38,3	35,5	32,8	82
10	1 30,1	1 23,4	1 17,5	1 12,1	1 7,2	1 2,6	58,4	54,5	50,8	47,4	44,2	41,1	38,1	35,3	32,6	80
12	1 29,5	1 22,8	1 17,0	1 11,6	1 6,7	1 2,2	58,0	54,1	50,5	47,0	43,9	40,8	37,8	35,0	32,4	78
14	1 28,8	1 22,2	1 16,4	1 11,0	1 6,2	1 1,7	57,5	53,7	50,1	46,7	43,6	40,5	37,5	34,7	32,1	76
16	1 28,0	1 21,4	1 15,6	1 10,4	1 5,6	1 1,2	57,0	53,2	49,6	46,2	43,2	40,1	37,2	34,4	31,8	74
18	1 27,0	1 20,5	1 14,8	1 9,6	1 4,9	1 0,5	56,4	52,6	49,1	45,7	42,7	39,6	36,8	34,0	31,5	72
20	1 26,0	1 19,7	1 13,9	1 8,8	1 4,1	59,8	55,7	52,0	48,5	45,2	42,2	39,2	36,3	33,6	31,1	70
22	1 24,9	1 18,5	1 12,9	1 7,9	1 3,2	59,0	55,0	51,3	47,9	44,6	41,6	38,6	35,9	33,2	30,7	68
24	1 23,6	1 17,4	1 11,9	1 6,9	1 2,3	58,2	54,2	50,5	47,1	43,9	41,0	38,1	35,3	32,7	30,2	66
26	1 22,3	1 16,1	1 10,7	1 5,8	1 1,3	57,2	53,3	49,7	46,4	43,2	40,3	37,4	34,8	32,2	29,8	64
28	1 20,9	1 14,8	1 9,5	1 4,7	1 0,2	56,2	52,4	48 9	45,6	42,5	39,6	36,8	34,2	31,6	29,2	62
30	1 19,3	1 13,3	1 8,1	1 3,4	59,1	55,2	51,4	47,9	44,8	41,6	38,9	36,1	33,5	31,0	28,7	60
32	1 17,7	1 11,7	1 6,7	1 2,1	57,9	54,0	50,3	46,9	43,8	40,8	38,1	35,3	32,8	30,3	28,1	58
34	1 15,8	1 10,2	1 5,2	1 0,7	56,6	52,7	49,2	45,9	42,8	39,9	37,2	34,5	32,0	29,6	27,5	56
36	1 14,0	1 8,5	1 3,6	59,3	55,2	51,5	48,0	44,8	41,8	38,9	36,3	33,7	31,3	28,9	26,8	54
38	1 12,1	1 6,7	1 1,9	57,7	53,8	50,1	46,8	43,6	40,7	37,9	35,4	32,8	30,4	28,2	26,1	52
40	1 10,1	1 4,8	1 0,2	56,1	52,3	48,8	45,5	42,4	39,6	36,9	34,4	31,9	29,6	27,4	25,4	50
42	1 8,0	1 2,8	58,4	54,4	50,7	47,3	44,1	41,2	38,4	35,8	33,3	30,9	28,7	26,5	24,6	48
44	1 5,9	1 0,8	56,5	52,7	49,1	45,8	42,7	39,9	37,2	34,6	32,3	29,9	27,8	25,7	23,8	46
46	1 3,4	58,7	54,6	50,9	47,4	44,2	41,3	38,5	35,8	33,4	31,2	28,9	26,8	24,8	23,0	44
48	1 1,1	56,7	52,7	49,0	45,7	42,5	39,8	37,0	34,5	32,2	30,0	27,8	25,9	23,9	22,2	42
50	58,8	54,4	50,6	47,1	43,9	40,9	37,8	35,5	33,2	30,9	28,8	26,8	24,9	22,9	21,3	40
52	56,3	52,1	48,4	45,1	42,1	39,2	36,5	34,1	31,8	29,7	27,6	25,7	23,8	22,0	20,4	38
54	53,8	49,7	46,2	43,1	40,2	37,4	34,9	32,5	30,4	28,3	26,4	24,5	22,7	21,0	19,5	36
56	51,2	47,3	44,0	41,0	38,2	35,6	33,2	31,0	28,9	26 9	25,1	23,3	21,6	19,9	18,6	34
58	48,4	44,9	41,7	38,9	36,2	33,7	31,5	29,4	27,4	25 5	23,8	22,1	20,5	18,9	17,6	32
60	45,8	42,3	39,3	36,6	34,1	31,8	29,6	27,6	25,8	24,0	22,5	20,8	19,3	17,9	16,5	30
62	42,9	39,7	36,9	34,4	32,1	29,9	27,9	26,0	24,2	22 5	21,1	19,5	18,1	16,8	15,5	28
64	40,1	37,1	34,4	32,1	30,1	27,9	26,1	24,3	22,7	21,0	19,7	18,2	16,9	15,7	14,5	26
66	37,2	34,4	31,9	29,8	27,8	25,9	24,2	22,6	21,0	19,5	18,3	16,9	15,7	14,5	13,4	24
68	34,3	31,6	29,4	27,4	25,6	23,8	22,2	20,8	19,4	18,0	16,9	15,5	14,4	13,4	12,4	22
70	31,3	29,0	26,9	25,1	23,4	21,7	20,4	18,9	17,6	16,4	15,4	14,3	13,2	12,2	11,3	20
72	28,3	26,2	24,3	22,7	21,1	19,7	18,3	17,1	16,0	14,8	13,9	12,9	12,0	11,0	10,2	18
74	25,3	23,3	21,7	20,2	18,8	17,6	16,4	15,3	14,3	13,2	12,4	11,5	10,6	9,8	9,1	16
76	22,2	20,5	19,0	17,8	16,6	15,4	14,4	13,4	12,5	11,6	10,9	10,1	9,3	8 6	8,0	14
78	19,0	17,0	16,3	15,3	14,2	13,2	12,4	11,5	10,8	10,0	9,4	8,6	8,0	7,4	6 9	12
80	15,9	14,6	13,6	12,8	11,9	11,0	10,3	9,6	8,9	8,3	7,8	7,2	6,7	6,1	5,7	10
82	12,8	11,7	10,9	10,3	9,6	8,9	8,3	7,8	7,2	6,7	6,3	5,7	5,4	5,0	4,6	8
84	9,6	8,8	8,2	7,7	7,2	6,7	6,3	5,8	5 4	5,0	4,7	4,4	4,0	3,8	3,4	6
86	6,5	5,8	5,4	5,2	4,8	4,5	4,2	3,9	3,7	3,4	3,1	2,9	2,6	2,5	2,3	4
88	3,2	2,9	2,7	2,6	2,4	2,3	2,1	2,0	1,9	1,7	1,6	1,5	1,4	1,3	1,2	2
90	0 0,0	0 0,0	0 0,0	0 0,0	0 0,0	0 0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0

Refraction in right ascension, × sec[t] declination.

This correction is − on the east, and + on the west side of the meridian.

And is to be applied to the sun or star's distance from the meridian, observed on the equatorial circle.

[*To face p.* 122.

No I. Table of the Effect of Refraction in North Polar Distance, continued.

This correction is always +.

Angle of the vertical with the horary circle.	DEGREES OF ALTITUDE.														Angle of the vertical with the horary circle.
	62°	64°	66°	68°	70°	72°	74°	76°	78°	80°	82°	84°	86°	88°	90°	
°	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	°
0	30,5	28,0	25,5	23,2	20,9	18,7	16,5	14,4	12,3	10,2	8,1	6,1	4,0	2,0	0,0	90
2	30,5	28,0	25,5	23,2	20,9	18,7	16,5	14,4	12,3	10,2	8,1	6,1	4,0	2,0		88
4	30,4	27,9	25,4	23,1	20,8	18,7	16,5	14,4	12,3	10,2	8,1	6,1	4,0	2,0		86
6	30,3	27,8	25,4	23,1	20,8	18,6	16,4	14,3	12,2	10,1	8,1	6,1	4,0	2,0		84
8	30,2	27,7	25,2	23,0	20,7	18,5	16,3	14,3	12,2	10,1	8,0	6,0	4,0	2,0		82
10	30,0	27,6	25,1	22,8	20,6	18,4	16,2	14,2	12,1	10,0	8,0	6,0	3,9	2,0		80
12	29,8	27,4	24,9	22,7	20,4	18,3	16,1	14,1	12,0	10,0	7,9	6,0	3,9	2,0		78
14	29,6	27,2	24,7	22,5	20,3	18,1	16,0	14,0	11,9	9,9	7,9	5,9	3,9	1,9		76
16	29,3	26,9	24,5	22,3	20,1	18,0	15,9	13,8	11,8	9,8	7,8	5,9	3,8	1,9		74
18	29,0	26,6	24,2	22,1	19,9	17,8	15,7	13,7	11,7	9,7	7,6	5,8	3,8	1,9		72
20	28,7	26,3	24,0	21,8	19,6	17,6	15,5	13,5	11,6	9,6	7,6	5,7	3,8	1,9		70
22	28,3	26,0	23,6	21,5	19,4	17,3	15,3	13,3	11,4	9,5	7,5	5,7	3,7	1,8		68
24	27,8	25,6	23,3	21,2	19,0	17,1	15,1	13,1	11,2	9,3	7,4	5,6	3,6	1,8		66
26	27,4	25,2	22,9	20,8	18,8	16,8	14,8	12,9	11,1	9,2	7,3	5,5	3,6	1,8		64
28	26,9	24,7	22,5	20,5	18,4	16,5	14,6	12,7	10,9	9,0	7,1	5,4	3,5	1,8		62
30	26,4	24,2	22,1	20,1	18,1	16,2	14,3	12,5	10,6	8,8	7,0	5,3	3,5	1,7		60
32	25,8	23,7	21,6	19,7	17,7	15,9	14,0	12,2	10,4	8,6	6,9	5,2	3,4	1,7		58
34	25,3	23,2	21,1	19,2	17,3	15,5	13,7	11,9	10,2	8,5	6,7	5,1	3,3	1,7		56
36	24,7	22,6	20,6	18,8	16,9	15,1	13,3	11,6	9,9	8,2	6,5	4,9	3,2	1,6		54
38	24,0	22,1	20,1	18,3	16,5	14,7	13,0	11,3	9,7	8,0	6,4	4,8	3,1	1,6		52
40	23,4	21,4	19,5	17,8	16,	14,3	12,6	11,0	9,4	7,8	6,2	4,7	3,1	1,5		50
42	22,7	20,8	18,9	17,2	15,5	13,9	12,3	10,7	9,1	7,6	6,0	4,5	3,0	1,5		48
44	22,0	20,1	18,3	16,7	15,0	13,4	11,9	10,4	8,8	7,3	5,8	4,4	2,9	1,4		46
46	21,1	19,4	17,7	16,1	14,5	13,0	11,5	10,0	8,5	7,1	5,6	4,2	2,8	1,4		44
48	20,4	18,7	17,1	15,5	14,0	12,5	11,0	9,6	8,2	6,8	5,4	4,1	2,7	1,3		42
50	19,6	18,0	16,4	14,9	13,4	12,0	10,6	9,3	7,9	6,6	5,2	3,9	2,6	1,3		40
52	18,8	17,2	15,7	14,3	12,9	11,4	10,2	8,9	7,6	6,3	5,0	3,8	2,5	1,2		38
54	17,9	16,5	15,0	13,6	12,3	11,0	9,7	8,5	7,2	6,0	4,8	3,6	2,3	1,2		36
56	17,1	15,7	14,3	13,0	11,7	10,5	9,2	8,0	6,9	5,7	4,5	3,4	2,2	1,1		34
58	16,2	14,8	13,5	12,3	11,1	9,9	8,7	7,6	6,5	5,4	4,3	3,2	2,1	1,1		32
60	15,2	14,0	12,7	11,6	10,4	9,3	8,2	7,2	6,1	5,1	4,0	3,0	2,0	1,0		30
62	14,3	13,1	12,0	10,9	9,8	8,8	7,7	6,8	5,8	4,8	3,8	2,9	1,9	0,9		28
64	13,4	12,3	11,2	10,2	9,2	8,2	7,2	6,3	5,4	4,5	3,5	2,7	1,7	0,9		26
66	12,4	11,4	10,4	9,4	8,5	7,6	6,7	5,9	5,0	4,1	3,3	2,5	1,6	0,8		24
68	11,4	10,5	9,5	8,7	7,8	7,0	6,2	5,4	4,6	3,8	3,0	2,3	1,5	0,7		22
70	10,5	9,6	8,7	7,9	7,1	6,4	5,6	4,9	4,2	3,5	2,8	2,1	1,4	0,7		20
72	9,4	8,6	7,9	7,2	6,5	5,6	5,1	4,4	3,8	3,1	2,5	1,9	1,2	0,6		18
74	8,4	7,7	7,0	6,4	5,8	5,1	4,5	4,0	3,4	2,8	2,2	1,7	1,1	0,5		16
76	7,4	6,8	6,2	5,6	5,1	4,5	4,0	3,5	3,0	2,5	2,0	1,5	1,0	0,5		14
78	6,3	5,8	5,3	4,8	4,3	3,9	3,4	3,0	2,6	2,1	1,7	1,3	0,8	0,4		12
80	5,3	4,9	4,4	4,0	3,6	3,2	2,9	2,5	2,1	1,8	1,4	1,1	0,7	0,3		10
82	4,3	3,9	3,5	3,2	2,9	2,6	2,3	2,0	1,7	1,4	1,1	0,8	0,6	0,3		8
84	3,2	2,9	2,7	2,4	2,2	1,9	1,7	1,5	1,3	1,1	0,8	0,6	0,4	0,2		6
86	2,2	1,9	1,8	1,6	1,5	1,3	1,1	1,0	0,9	0,7	0,6	0,4	0,3	0,1		4
88	1,1	1,0	0,9	0,8	0,7	0,6	0,6	0,5	0,4	0,4	0,3	0,2	0,1	0,1		2
90	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0		0

Refraction in right ascension, × sec[t] declination.

This correction is − on the east, and + on the west side of the meridian.

No. II. Table of the Effect of Parallax in North Polar Distance, and AR.

The horizontal parallax = 8″,6. This correction is always —.

Angle of the vertical and horary.	DEGREES OF ALTITUDE.										Angle of the vertical and horary.
	0°	10°	20°	30°	40°	50°	60°	70°	80°	90°	
°	″	″	″	″	″	″	″	″	″	″	°
0	8,60	8,47	8,08	7,45	6,59	5,53	4,30	2,94	1,49	0,0	90
10	8,47	8,34	7,96	7,34	6,49	5,45	4,23	2,90	1,47	0,0	80
20	8,08	7,96	7,60	7,00	6,19	5,20	4,04	2,77	1,40	0,0	70
30	7,45	7,34	6,99	6,45	5,71	4,79	3,72	2,55	1,29	0,0	60
40	6,59	6,49	6,18	5,70	5,05	4,23	3,29	2,26	1,15	0,0	50
50	5,53	5,44	5,19	4,79	4,23	3,55	2,76	1,89	0,96	0,0	40
60	4,30	4,23	4,04	3,72	3,30	2,76	2,15	1,47	0,74	0,0	30
70	2,94	2,90	2,77	2,55	2,26	1,89	1,47	1,01	0,51	0,0	20
80	1,49	1,47	1,40	1,29	1,14	0,95	0,75	0,50	0,26	0,0	10

Parallax in right ascension, × sec^t declination.

This correction is + on the east, and — on the west side of the meridian.

No. III. Table of the Effect of Parallax in North Polar Distance, and AR.

The horizontal parallax being = 10″. This correction is always —.

Angle of the vertical and horary.	DEGREES OF ALTITUDE.										Angle of the vertical and horary.
	0°	10°	20°	30°	40°	50°	60°	70°	80°	90°	
°	″	″	″	″	″	″	″	″	″	″	°
0	10,00	9,85	9,40	8,66	7,66	6,43	5,00	3,42	1,73	0,0	90
10	9,85	9,70	9,26	8,53	7,54	6,33	4,92	3,37	1,70	0,0	80
20	9,40	9,25	8,83	8,14	7,20	6,04	4,70	3,21	1,63	0,0	70
30	8,66	8,53	8,14	7,51	6,64	5,57	4,33	2,96	1,50	0,0	60
40	7,66	7,54	7,20	6,63	5,87	4,93	3,83	2,62	1,32	0,0	50
50	6,43	6,33	6,04	5,56	4,92	4,13	3,21	2,21	1,11	0,0	40
60	5,00	4,92	4,70	4,33	3,83	3,21	2,50	1,71	0,86	0,0	30
70	3,42	3,37	3,21	2,96	2,62	2,20	1,71	1,17	0,59	0,0	20
80	1,73	1,71	1,63	1,50	1,33	1,11	0,87	0,59	0,30	0,0	10

Parallax in right ascension, × sec^t declination.

This correction is + on the east, and — on the west side of the meridian.

No. IV.

Table of Natural Secants.

Deg.	Nat. sec.	Deg.	Nat. sec.	Deg.	Nat. sec.	Deg.	Nat. sec.	Deg.	Nat. sec.	Deg.	Nat. sec.
1	10002	16	10403	31	11666	46	14396	61	20627	76	41336
2	10006	17	10457	32	11792	47	14663	62	21301	77	44454
3	10014	18	10515	33	11924	48	14945	63	22027	78	48097
4	10024	19	10576	34	12062	49	15243	64	22812	79	52408
5	10038	20	10642	35	12208	50	15557	65	23662	80	57588
6	10055	21	10711	36	12361	51	15890	66	24586	81	63925
7	10075	22	10785	37	12521	52	16243	67	25593	82	71853
8	10098	23	10864	38	12690	53	16626	68	26695	83	82055
9	10124	24	10946	39	12868	54	17013	69	27904	84	95668
10	10154	25	11034	40	13054	55	17434	70	29238	85	114737
11	10187	26	11126	41	13250	56	17883	71	30716	86	143356
12	10223	27	11223	42	13456	57	18361	72	32361	87	191075
13	10263	28	11326	43	13673	58	18871	73	34203	88	286537
14	10306	29	11434	44	13902	59	19416	74	36280	89	572987
15	10353	30	11547	45	14142	60	20000	75	38637	90	Infinite

No. V. Table of the Correction of the Time, shewn by an Equatorial, on Account of Refraction, when the Instrument is not previously adjusted to the true Meridian.

Angle of the vertical with the horary circle.	DEGREES OF ALTITUDE.											
	7°	10°	15°	20°	25°	30°	35°	40°	45°	50°	60°	70°	80°
0	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.
5	338,	241,	161,3	118,9	94,2	74,9	62,	51,7	43,7	36,8	25,3	16,1	8,
10	169,	120,	80,7	59,4	47,	37,4	31,	25,9	21,9	18,4	12,6	8,1	4,
15	114,	81,	54,3	40,	31,8	25,2	20,9	17,4	14,7	12,4	8,5	5,4	2,7
20	86,	61,	41,	30,4	24,1	19,1	15,8	13,2	11,2	9,4	6,5	4,1	2,1
25	70,	50,	33,4	24,6	19,5	15,5	12,8	10,7	9,1	7,6	5,2	3,3	1,7
30	59,	42,	28,2	20,6	16,4	13,	10,8	9,	7,6	6,4	4,4	2,8	1,4
35	51,	36,	24,4	18,	14,3	11,4	9,4	7,8	6,6	5,6	3,8	2,4	1,2
40	46,	33,	21,8	16,	12,7	10,1	8,4	7,	5,9	5,	3,4	2,2	1,1
45	41,	30,	19,9	14,6	11,8	9,2	7,6	6,3	5,4	4,5	3,1	2,	1,
50	38,	27,	18,3	13,4	10,7	8,5	7,1	5,9	5,	4,2	2,9	1,8	0,
55	36,	26,	17,1	12,6	10,1	7,9	6,6	5,5	4,6	3,9	2,7	1,7	0,9
60	34,	24,	16,2	11,9	9,5	7,5	6,3	5,2	4,4	3,7	2,5	1,6	0,8
65	32,	23,	15,5	11,4	9,1	7,2	6,	4,9	4,2	3,5	2,4	1,5	0,8
70	31,	22,	14,9	11,	8,8	6,9	5,8	4,8	4,	3,4	2,3	1,5	0,7
80	30,	21,	14,2	10,5	8,3	6,6	5,5	4,6	3,9	3,2	2,2	1,4	0,7
90	29,	21,	14,	10,3	8,2	6,5	5,4	4,5	3,8	3,2	2,2	1,4	0,7

× Secant of declination.

This equation is − on the east, and + on the west side of the meridian.

No. VI. Table shewing the Correction of the Meridian Line, found by an Equatorial, arising from the Effect of Refraction, in Minutes and Decimals.

Angle of the vertical with the horary circle.	DEGREES OF ALTITUDE.												
	7°	10°	15°	20°	25°	30°	35°	40°	45°	50°	60°	70°	80°
0	'	'	'	'	'	'	'	'	'	'	'	'	'
5	84,5	60,3	40,	29,8	22,9	18,3	15,5	12,6	10,9	9,2	6,3	4,	2,3
10	41,7	29,7	19,8	14,8	11,4	9,1	7,7	6,2	5,4	4,5	3,1	2,	1,1
15	27,3	19,5	13,1	9,7	7,5	5,9	5,	4,1	3,5	3,	2,	1,3	0,7
20	20,1	14,4	9,6	7,2	5,5	4,4	3,7	3,	2,6	2,2	1,5	1,	0,5
25	15,8	11,4	7,6	5,6	4,3	3,4	2,9	2,4	2,	1,7	1,2	0,8	0,4
30	12,7	9,1	6,1	4,5	3,4	2,8	2,3	1,9	1,6	1,4	0,9	0,6	0,3
35	10,2	7,5	5,	3,7	2,8	2,3	1,9	1,6	1,3	1,1	0,8	0,5	0,3
40	8,7	6,2	4,2	3,1	2,4	1,9	1,6	1,3	1,1	0,9	0,6	0,4	0,2
45	7,3	5,2	3,5	2,6	2,	1,6	1,3	1,1	0,9	0,8	0,5	0,3	0,2
50	6,1	4,4	2,9	2,2	1,7	1,3	1,1	0,9	0,8	0,6	0,4	0,3	0,2
55	5,1	3,7	2,4	1,8	1,4	1,1	0,9	0,8	0,7	0,5	0,4	0,2	0,1
60	4,2	3,	2,	1,5	1,1	0,9	0,8	0,6	0,5	0,4	0,3	0,2	0,1
65	3,4	2,4	1,6	1,2	0,9	0,8	0,6	0,5	0,4	0,3	0,2	0,2	0,1
70	2,7	1,9	1,3	0,9	0,7	0,6	0,5	0,4	0,3	0,3	0,2	0,1	0,1
80	1,3	0,9	0,6	0,5	0,4	0,3	0,2	0,2	0,2	0,1	0,1	0,1	0,
90	0,0	0,	0,	0,	0,	0,	0,	0,	0,	0,	0,	0,	0,

× Secant of the altitude.

Note. If the observation is on the {east / west} side of the meridian, then is the true meridian so many minutes to the {west / east} of that found by the instrument.

No. VII. Table of the Effect of Refraction in Right Ascension in Time, when the Equatorial is adjusted to the Meridian.

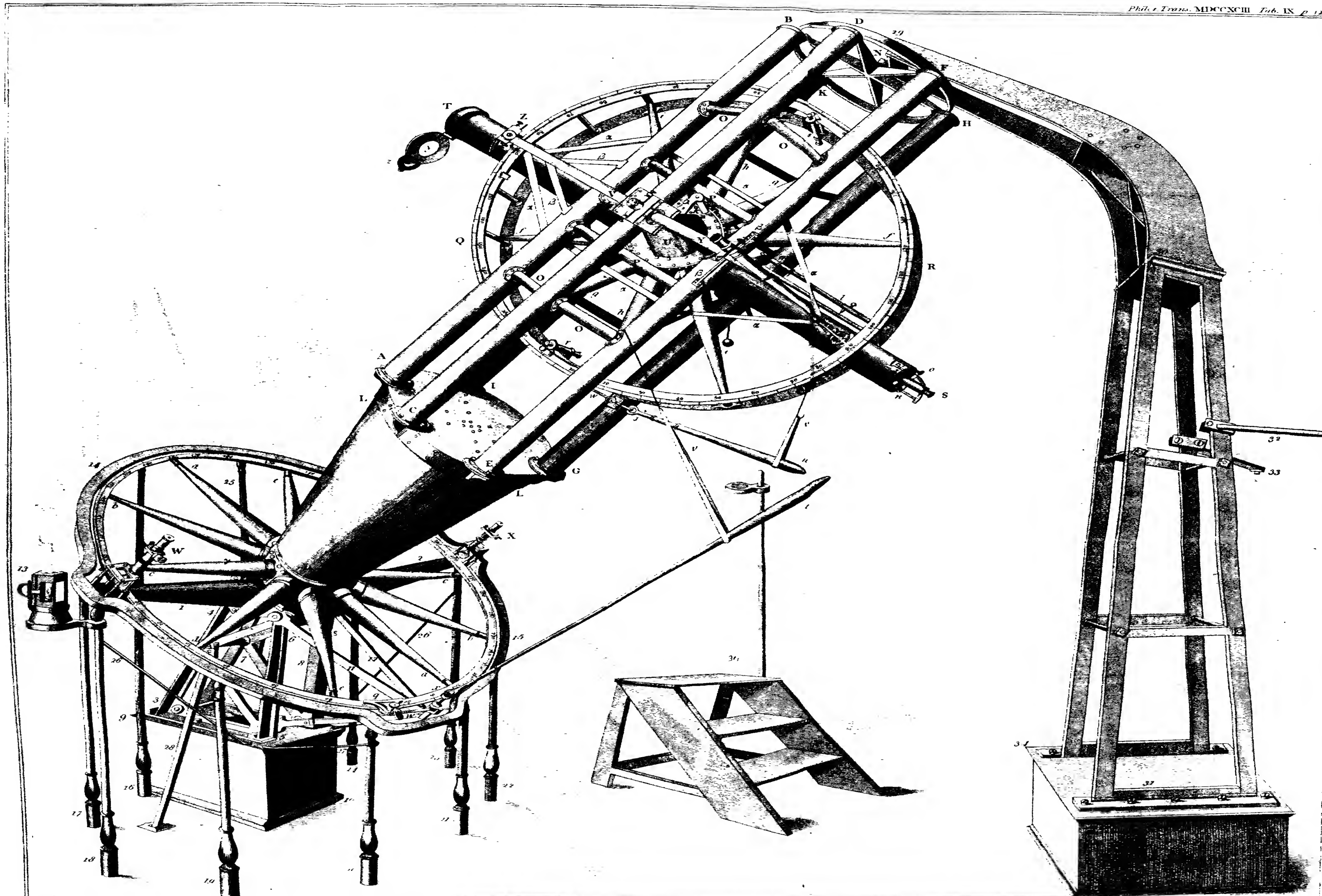
Angle of the vertical with the horary circle.	DEGREES OF ALTITUDE.														
	3°	5°	7°	10°	15°	20°	25°	30°	35°	40°	45°	50°	60°	70°	80°
0	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.	Sec.
5	5,1	3,5	2,5	1,8	1,2	0,9	0,7	0,6	0,5	0,4	0,3	0,3	0,2	0,1	0,1
10	10,1	6,9	5,1	3,7	2,5	1,8	1,4	1,1	0,9	0,8	0,7	0,5	0,4	0,3	0,1
15	15,	10,3	7,6	5,4	3,6	2,7	2,1	1,7	1,4	1,2	1,	0,8	0,6	0,4	0,2
20	19,9	13,5	10,	7,2	4,8	3,5	2,8	2,2	1,9	1,4	1,2	1,1	0,7	0,5	0,2
25	24,5	16,6	12,3	8,8	5,9	4,3	3,4	2,7	2,3	1,9	1,6	1,3	0,9	0,6	0,3
30	29,	19,7	14,6	10,5	7,	5,2	4,1	3,3	2,7	2,3	1,9	1,7	1,1	0,7	0,3
35	33,4	22,7	16,8	12,	8,	5,9	4,7	3,7	3,1	2,6	2,2	1,8	1,3	0,8	0,4
40	37,4	25,4	18,6	13,5	9,	6,7	5,2	4,2	3,4	2,9	2,4	2,1	1,4	0,9	0,4
45	41,3	28,	20,7	14,9	9,9	7,3	5,7	4,6	3,9	3,2	2,7	2,3	1,5	1,	0,5
50	44,7	30,3	22,5	16,1	10,7	7,9	6,2	5,	4,1	3,5	2,9	2,5	1,7	1,1	0,5
55	47,7	32,4	24,	17,2	11,5	8,5	6,7	5,3	4,4	3,7	3,1	2,7	1,8	1,1	0,5
60	50,3	34,2	25,3	18,1	12,1	8,9	7,	5,7	4,7	3,9	3,3	2,8	1,9	1,2	0,6
65	52,8	35,9	26,5	19,	12,8	9,3	7,4	5,9	4,9	4,2	3,5	2,9	2,	1,3	0,6
70	54,6	37,	27,5	19,7	13,1	9,7	7,7	6,1	5,1	4,3	3,5	3,	2,1	1,3	0,6
80	57,6	39,1	28,9	20,7	13,8	10,1	8,	6,5	5,3	4,5	3,8	3,2	2,2	1,4	0,7
90	58,4	39,6	29,3	21,	14,	10,3	8,2	6,5	5,4	4,5	3,8	3,2	2,2	1,4	0,7

× Secant of declination.

This correction is — on the east, and + on the west side of the meridian.

No. VIII. Table of the Effect of Refraction in Declination, when the Equatorial is adjusted to the Meridian.

Angle of the vertical with the horary circle.	DEGREES OF ALTITUDE.														
	3°	5°	7°	10°	15°	20°	25°	30°	35°	40°	45°	50°	60°	70°	80°
°	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	′ ″	″	″	″	″	″
0	14 36	9 54	7 20	5 15	3 30	2 35	2 2	1 38	1 21	1 8	57	48	33	21	10
10	14 24	9 46	7 14	5 11	3 27	2 32	2	1 37	1 20	1 7	57	48	33	21	10
20	13 39	9 16	6 52	4 56	3 17	2 26	1 55	1 32	1 16	1 4	53	45	31	20	9
25	13 12	8 59	6 38	4 45	3 10	2 20	1 50	1 29	1 14	1 2	52	44	30	19	9
30	12 35	8 33	6 20	4 32	3 1	2 14	1 45	1 25	1 10	59	49	42	29	18	9
35	11 55	8 6	6	4 18	2 52	2 7	1 40	1 20	1 6	56	46	39	27	17	8
40	11 11	7 35	5 37	4 1	2 40	1 59	1 33	1 15	1 2	52	43	37	25	16	8
45	10 19	7	5 11	3 43	2 28	1 50	1 26	1 9	58	48	40	34	23	15	7
50	9 22	6 21	4 42	3 22	2 15	1 40	1 18	1 3	52	44	36	31	21	14	6
55	8 21	5 40	4 12	3	2	1 29	1 10	56	46	39	33	27	19	12	6
60	7 16	4 56	3 39	2 37	1 45	1 18	1 1	49	40	34	29	24	16	11	5
65	6 7	4 9	3 4	2 12	1 28	1 5	51	41	34	28	24	20	14	9	4
70	4 58	3 22	2 30	1 48	1 12	53	42	33	28	23	19	16	11	7	3
75	3 45	2 34	1 54	1 21	54	40	32	25	21	18	15	12	9	6	3
80	2 32	1 43	1 16	55	37	27	21	17	14	12	10	8	6	4	2
85	1 16	52	38	27	18	13	11	9	7	6	5	4	3	2	1



GENERAL VIEW OF THE EQUATORIAL INSTRUMENT.

One third the size of the Original.

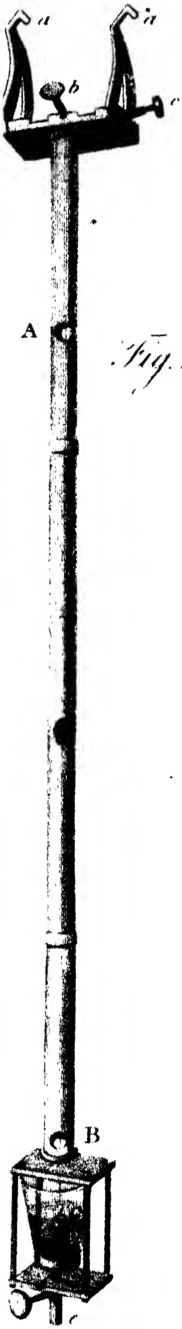


Fig. 3.

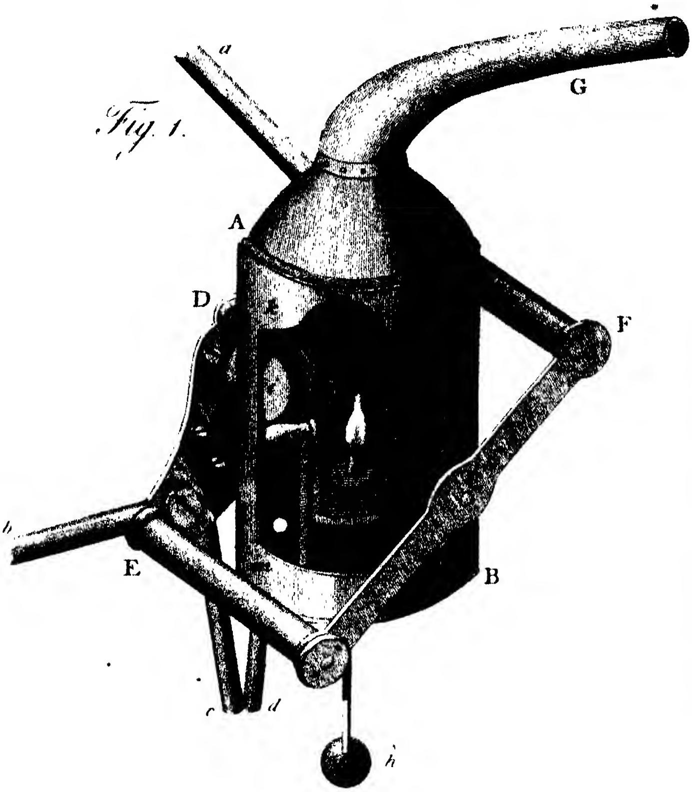


Fig. 1.

One half the size of the Original

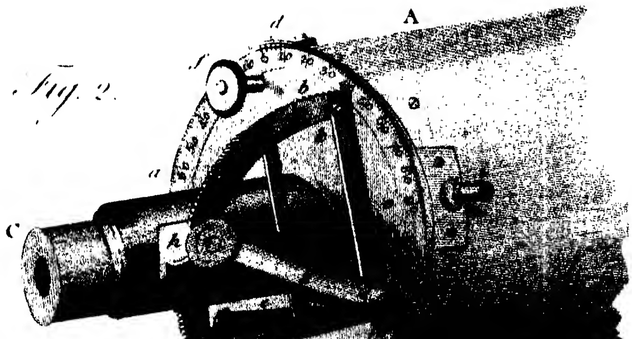
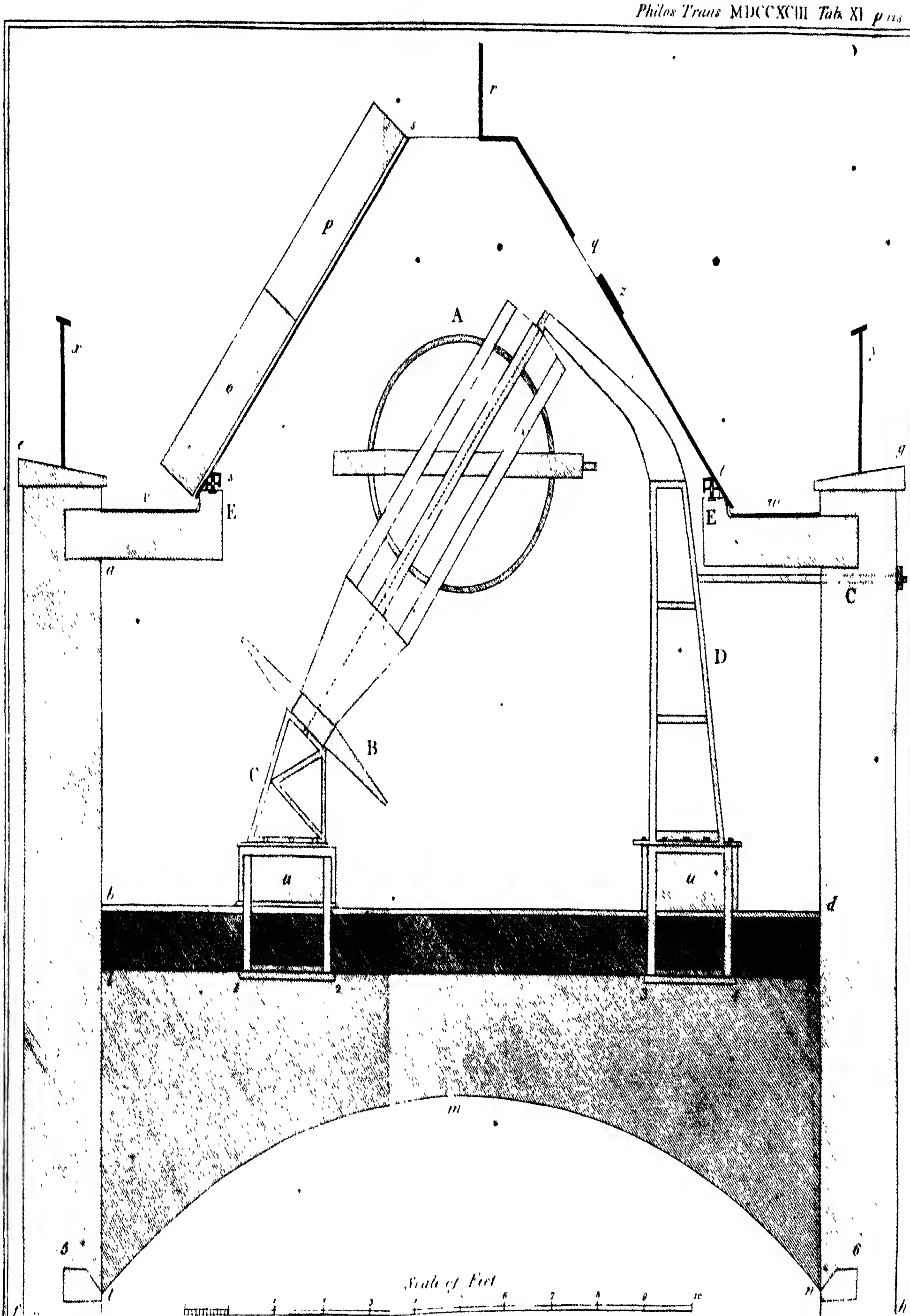


Fig. 2.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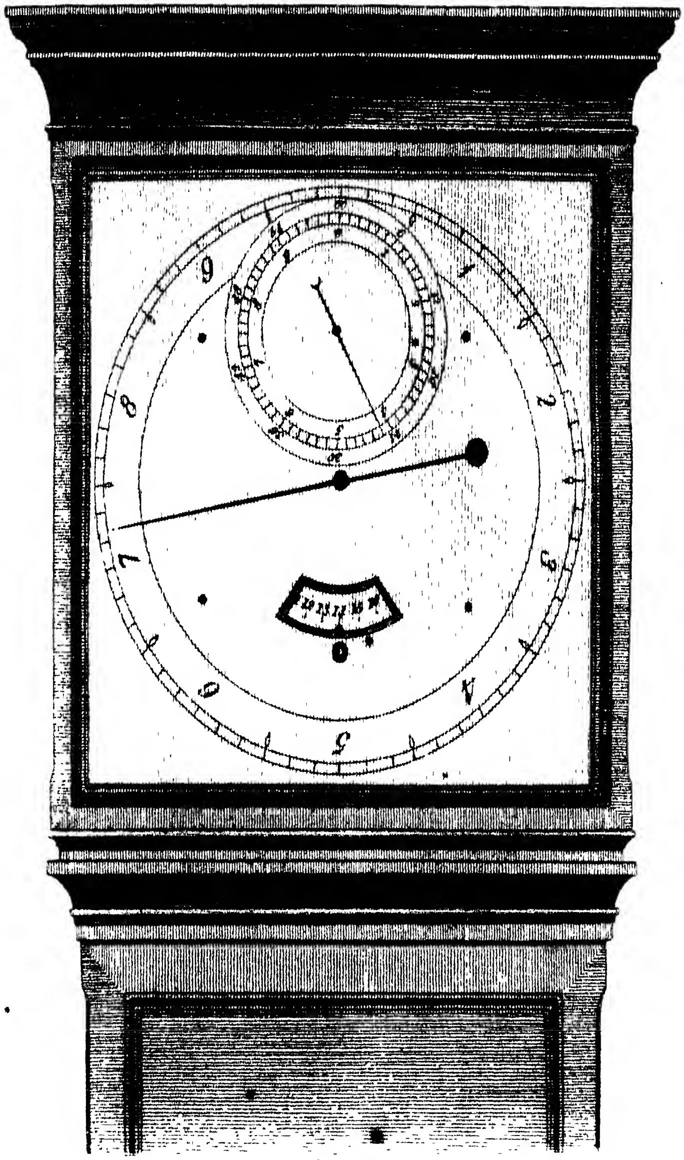

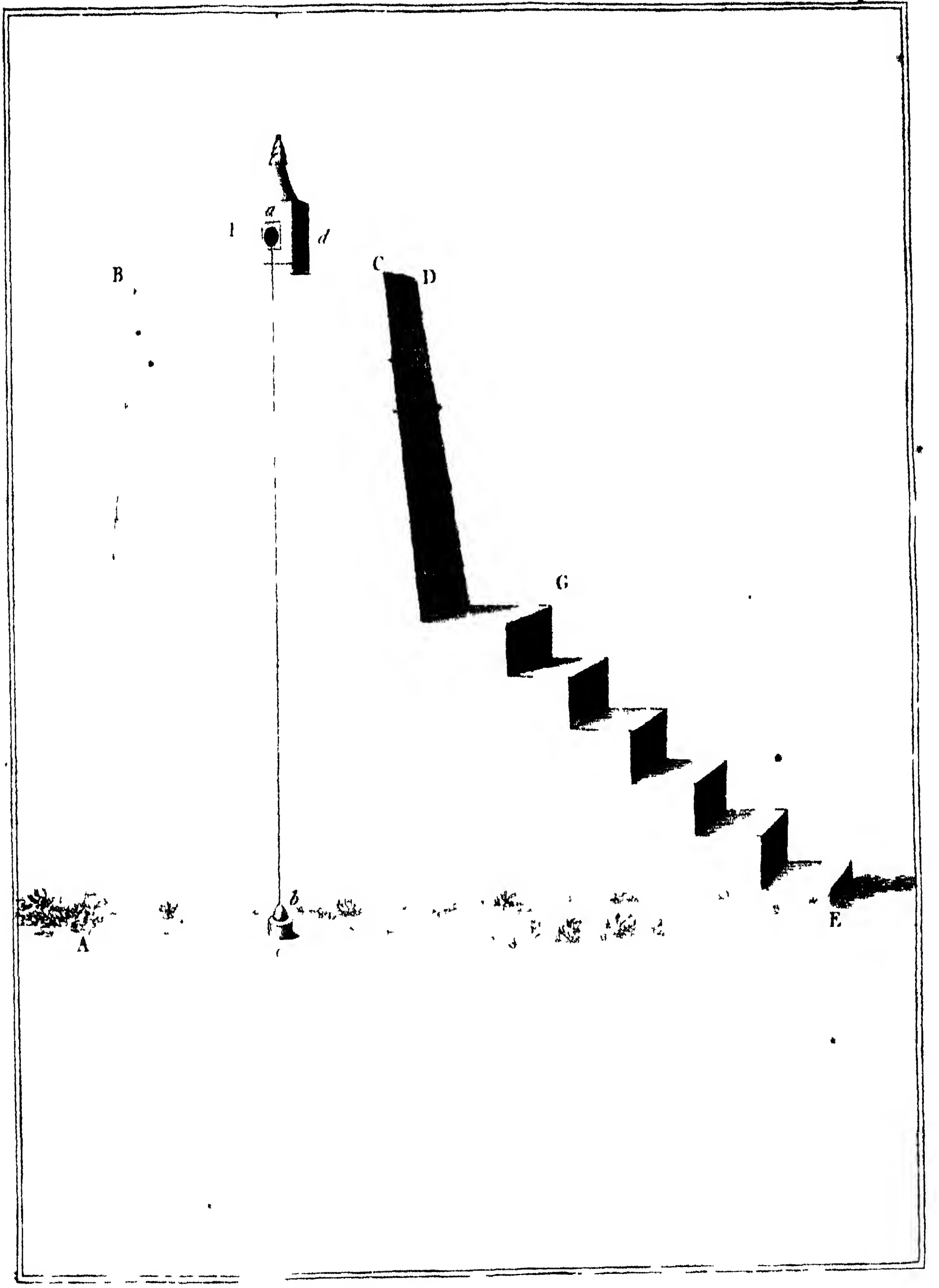
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Fig 1 ⅓ of the original size

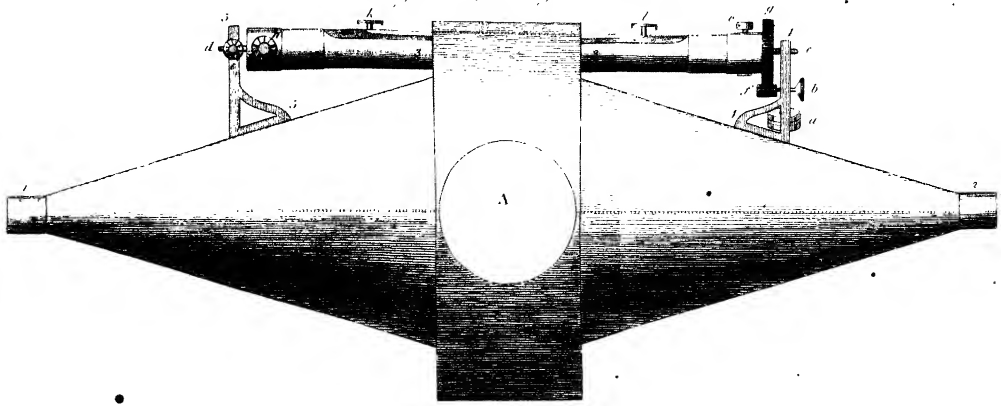
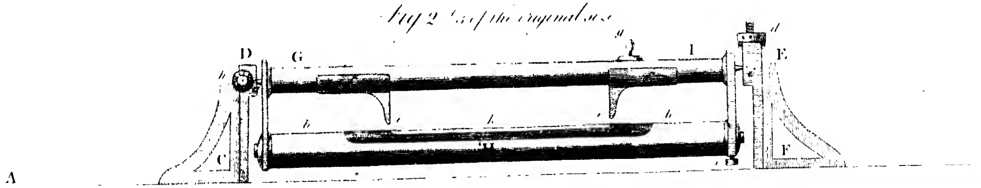


Fig 2 ⅓ of the original size



XI. *Additional Observations on the Method of making Ice at* Benares. *In a Letter to* William Marsden, *Esq. F. R. S. from* John Lloyd Williams, *Esq. of* Benares.

Read May 2, 1793.

DEAR SIR,

In addition to what I have already communicated to you, respecting the mode of procuring ice in this country, the following observations on that subject, accompanied with some account of the temperature of the air, and state of the thermometer, may not be unacceptable.

April 30th, 1792, the thermometer, in the shade, being at 95 degrees, some water was taken up from a well, sixty feet deep, and the thermometer being immerged in it, its temperature was found to be 74 degrees. This water was then poured into four pots, or pans, similar to those which, in my former letter, I mentioned as being employed in the process for making ice. They were also similar to each other in size and construction, except that two of them were new and unglazed, and the two others old, with their pores closed, so that no moisture could transpire through them. These pots were then exposed to a hot westerly wind, in the shade, for the space of three hours; viz. from two o'clock in the afternoon till five. Upon examining them at that time, the water in the old pots was found to be at 84 degrees, and that in the new, or porous ones, at 68. After remaining in that situation one hour longer, the water in the old pots rose to 88 degrees, whilst that in the new ones continued at 68.

May 1st, at two o'clock in the afternoon, the thermometer then being, in the sun, at 110 degrees, and in the shade at 100, the experiment was repeated, with the same pots as before. After being filled with well-water, they were exposed for four hours, viz. from two o'clock till six, to a hot wind; the water in the old pots was then found to be at 97 degrees, that in the new ones at 68.

The foregoing observations on the frigorific effect of evaporation from porous vessels, will perhaps account, in some measure, for ice being formed when the thermometer, in the air, is above the freezing point. And the power of evaporation in generating cold, may be further elucidated by the following observations on the effects produced, by its means, in our houses.

May 16, 1792, at two in the afternoon,

The thermometer, in the sun, with a hot westerly wind, rose to - - - - -	118 degrees.
Ditto, in the shade, but exposed to the hot wind - - - - -	110 ditto.
Ditto, in the house, which was kept cool by *tatties* - - - - -	87 ditto.

June 7.

Thermometer, in the sun - - -	113 degrees.
Ditto, in the shade, and hot wind -	104 ditto.
Ditto, in the house, cooled by *tatties* -	83 ditto.

Tatties are a kind of mat, made of fresh green bushes, or long roots, like snake-root; they are affixed to the door or window frames, and kept constantly sprinkled with water. The degree of cold produced by their means is supposed to be in proportion to the heat of the wind which passes through them, as on that depends the quantity of evaporation.

I am, &c.

Benares,
October 1, 1792.

J. LL. WILLIAMS.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1792.

1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer*	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Jan. 1	8	0	44	53	29,34	80	0,105	SSE	1	Cloudy.
	2	0	44	57,5	29,38	78		E	1	Cloudy.
2	8	0	41	52	29,87	83	0,125	NE	1	Rain.
	2	0	43,5	55	29,95	74		NNE	2	Cloudy.
3	8	0	36,5	52	30,14	73		N	1	Cloudy.
	2	0	43	55	30,14	73		NE	1	Cloudy.
4	8	0	38	52	30,35	77		NNE	1	[illegible]y.
	2	0	42	55,5	30,38	73		NNE	1	Cloudy.
5	8	0	38	52	30,47	77		N	1	Cloudy.
	2	0	39,5	54,5	30,46	71		NNW	1	Cloudy.
6	8	0	35	52	30,42	75		NNE	1	Cloudy.
	2	0	38,5	54,5	30,33	70		NN[illegible]	1	Cloudy.
7	8	0	40	51,5	30,11	77		W	1	Cloudy.
	2	0	44	55	30,00	72		WNW	1	Cloudy.
8	8	0	31	50	30,15	69		NE	1	Cloudy.
	2	0	36	54,5	30,18	65		NE	1	Fine.
9	8	0	33	50	29,80	70		WNW	1	Cloudy.
	2	0	38	53,5	29,55	75		WNW	1	Cloudy.
10	8	0	34,5	50	29,36	73	0,060	WNW	1	Cloudy.
	2	0	36	53,5	29,36	75		E	1	Cloudy.
11	8	0	25	46	29,27	68		NE	1	Fair.
	2	0	29	48	29,21	60		NE	1	Fine.
12	8	0	19	44	29,21	67		WNW	1	Fine.
	2	0	28	46,5	29,25	65		WNW	1	Fine.
13	8	0	20	44	29,64	69		WNW	1	Hazy.
	2	0	28	45,5	29,71	63		WNW	1	Fine.
14	8	0	24	43	29,67	70		E	2	Fine.
	2	0	30	44,5	29,44	64		E	2	Cloudy.
15	8	0	40	44,5	29,06	92	0,040	S	2	Rain.
	2	0	43	48	28,98	85		SSE	2	Cloudy.
16	8	0	41	46,5	29,11	82	0,015	S	2	Cloudy.
	2	0	43	49,5	29,13	81		S	2	Rain.

* These observations were made with Mr. De Luc's hygrometer, described in Vol. LXXXI. of the Phil. Trans. p. 420.

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for January, 1792.

1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Jan. 17	8	0	42	48,5	29,25	*	0,074	ENE	1	Foggy.
	2	0	45	52	29,42			NNE	1	Cloudy.
18	8	0	34	48,5	29,88			NE	1	Fair.
	2	0	37	51,5	29,92			NE	1	Fair.
19	8	0	32	48	30,00			NE	1	Cloudy.
	2	0	36	49,5	30,00			ENE	1	Cloudy.
20	8	0	33	48	30,04			ENE	1	Cloudy.
	2	0	34	48	29,96			E	1	Cloudy.
21	8	0	32	47	29,80			E	1	Cloudy.
	2	0	36	49	29,83			E	1	Cloudy.
22	8	0	35	48	29,85			E	1	Foggy.
	2	0	40	50	29,75			E	1	Cloudy.
23	8	0	33	47	29,47		0,268	WNW	1	Cloudy.
	2	0	36,5	50	29,56			WNW	1	Cloudy.
24	8	0	42	48	29,57		0,055	SSW	1	Foggy.
	2	0	47	51	29,46			S	2	Cloudy.
25	8	0	47	51	29,20		0,340	SW	2	Cloudy; much wind
	2	0	51	55	29,32			SW	2	Cloudy. [last night.
26	8	0	48	52,5	29,01		0,145	SW	2	Cloudy; much wind
	2	0	50	54,5	28,94			SW	2	Cloudy. [last night.
27	8	0	46	53,5	29,48		0,130	WSW	1	Cloudy.
	2	0	50	57,5	29,57			W	2	Cloudy.
28	8	0	42	53	29,52			E	1	Cloudy.
	2	0	48	57	29,35			SW	1	Rain.
29	8	0	46	55	29,55		0,195	SSW	1	Cloudy.
	2	0	48	57	29,46			SSW	1	Cloudy.
30	8	0	43	55	29,75		0,120	SW	1	Cloudy.
	2	0	50	57	29,74			SW	2	Cloudy.
31	8	0	48	55	29,52			SW	1	Fair.
	2	0	53	57,5	29,53		0,138	SW	2	Fine.

* This interruption, in the observations of the hygrometer, was occasioned by one end of the whalebone having slipped out of the pincers by which it is held.

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for February, 1792.

1792	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
	H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Feb. 1	8	0	47	56	29,66			SW	1	Fair.
	2	0	52	58	29,80			WNW	1	Fair.
2	8	0	50	56	29,90			SW	2	Cloudy.
	2	0	52,5	58	29,84			SW	2	Cloudy.
3	8	0	40	56	30,05			W	1	Fine.
	2	0	47	57	30,10			W	2	Cloudy.
4	8	0	35	55	30,29			W	1	Fair.
	2	0	43	58,5	30,29			NW	1	Fine.
5	8	0	37	54	30,17			E	1	Foggy.
	2	0	43	55	30,13			ESE	1	Fair.
6	8	0	37	53	30,00			E	1	Cloudy.
	2	0	45	55	29,95			SSW	1	Cloudy.
7	8	0	48	53,5	29,75		0,095	SW	2	Cloudy.
	2	0	51,5	56,5	29,70			SW	2	Cloudy.
8	8	0	43	54	29,67		0,295	WSW	1	Fair.
	2	0	49,5	57	29,83			WSW	1	Fair.
9	8	0	40	55	30,30			N	1	Fair.
	2	0	43	56,5	30,40			W	1	Cloudy.
10	8	0	43	54,5	30,40			SW	1	Cloudy.
	2	0	48	57	30,36			WSW	1	Cloudy.
11	8	0	44	55	30,18			SWbyW	1	Cloudy.
	2	0	50	58	30,16			WSW	1	Cloudy.
12	8	0	46	56	30,16			WSW	1	Fair.
	2	0	56	58,5	30,18			WSW	1	Fine.
13	8	0	41	57	30,26			WSW	1	Fine.
	2	0	52	60	30,26			WSW	1	Fine.
14	8	0	37	56	30,22			WNW	1	Fair.
	2	0	44	58	30,22			WNW	1	Hazy.
15	8	0	37	55	30,06			W	1	Foggy.
	2	0	45	57,5	30,04			E	1	Fair.
16	8	0	36	54	30,17			NE	1	Cloudy.
	2	0	38	56	30,22			NE	1	Cloudy.

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1792	Time. H.	M	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
Feb. 17	8	0	26	51	30,40			NNE	2	Fair.
	2	0	32	53	30,28			NE	2	Fair.
18	8	0	23	47	29,98			NNE	2	Fair.
	2	0	26,5	49,5	30,00			NNE	2	Fine.
19	8	0	30	46,5	29,70			NNE	2	Cloudy.
	2	0	32	48,5	29,62			E	1	Snow.
20	8	0	22,5	44	29,53			NE	1	Snow.
	2	0	26	47	29,63	49		NNW	1	Fine.
21	8	0	16,5	43	29,78	59		WNW	1	Hazy.
	2	0	29	46	29,77	52		W	1	Fair.
22	8	0	31	43,5	29,59	73		NW	1	Hazy.
	2	0	34,5	48	29,72	59		WNW	1	Fair.
23	8	0	27	44	29,88	67		W	1	Hazy.
	2	0	33	48	29,93	60		E	1	Fine.
24	8	0	30	44,5	29,86	65		E	1	Cloudy.
	2	0	37	47	29,80	61		E	1	Rain.
25	8	0	38,5	46	29,84	90	0,120	E	1	Cloudy.
	2	0	41,5	47,5	29,77	87		E	1	Cloudy.
26	8	0	43,5	48	29,61	90	0,202	SE	1	Cloudy.
	2	0	51	51	29,64	79		SSW	2	Cloudy.
27	8	0	42	49	29,89	86		SW	2	Fine.
	2	0	49	52,5	29,94	68		SW	1	Cloudy.
28	8	0	40	50	30,02	82		ENE	1	Cloudy.
	2	0	49	53	30,04	64		ESE	1	Hazy.
29	8	0	40	52	29,99	74		E	1	Cloudy.
	2	0	47	54	29,95	71		SE	1	Cloudy.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Mar. 1	7	0	43	52	29,80	85		E	1	Foggy.
	2	0	50	55	29,66	64		ESE	2	Cloudy.
2	7	0	47	52,5	29,45	70		E	1	Cloudy.
	2	0	51	56	29,45	79		S	1	Rain.
3	7	0	43	55	29,58	84	0,050	SSW	1	Cloudy.
	2	0	50	57	29,57	64		SSW	2	Cloudy.
4	7	0	48	54	29,14	79	0,091	S	2	Rain.
	2	0	50	57	29,07	77		SSW	2	Cloudy.
5	7	0	45	55	29,40	77	0,510	SW	1	Cloudy.
	2	0	46	56	29,58	63		WNW	1	Cloudy.
6	7	0	41	53	29,44	75		S	2	Rain.
	2	0	48	53	29,37	53		WSW	2	Fine.
7	7	0	37	53,5	29,44	70	0,129	W	1	Fine.
	2	0	40	55	29,50	67		WSW	1	Rain.
8	7	0	34	52	29,57	63	0,231	WNW	1	Fair.
	2	0	40	54	29,64	55		NW	1	Cloudy.
9	7	0	26	50	29,91	64		NNE	1	Fair.
	2	0	33	52	29,98	55		NNE	1	Cloudy.
10	7	0	26	48,5	30,13	65		NNE	1	Fine.
	2	0	34	48	30,18	54		NNE	1	Fine.
11	7	0	28	46,5	30,26	64		NNE	1	Fine.
	2	0	36	48,5	30,31	51		NNE	1	Fair.
12	7	0	29	47	30,49	63		ENE	1	Fair.
	2	0	36	51	30,51	51		E	1	Fine.
13	7	0	30	46	30,23	61		E	1	Cloudy.
	2	0	40	48,5	29,91	53		SE	1	Cloudy.
14	7	0	41	47	29,58	86	0,067	SW	1	Cloudy.
	2	0	49	51	29,54	57		WSW	1	Cloudy.
15	7	0	43	49	29,41	78	0,134	WNW	1	Fair.
	2	0	47	52	29,40	61		W	1	Cloudy.
16	7	0	38	50	29,86	76		W	1	Fair.
	2	0	51	54	29,93	65		W	1	Fair.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Mar. 17	7	0	47	54	29,83	79		SW	2	Cloudy.
	2	0	52	56	29,82	74		SW	2	Cloudy.
18	7	0	49	54	29,66	70		SW	2	Cloudy.
	2	0	53,5	57	29,75	57		SW	2	Fair.
19	7	0	38	53,5	30,12	70	0,034	W	1	Fine.
	2	0	50	58	30,19	48		W	1	Fine.
20	7	0	42	53,5	30,19	65		S	2	Fine.
	2	0	50	57	30,12	49		S by W	2	Fine.
21	7	0	43	54	30,08	76	0,041	SW	1	Cloudy.
	2	0	53	57,5	30,11	53		WNW	1	Cloudy.
22	7	0	43	54,5	30,08	56		WNW	1	Cloudy.
	2	0	51	57,5	30,05	57		NW	1	Fair.
23	7	0	43	55	29,87	61		SW	2	Cloudy.
	2	0	53,5	58	29,82	54		WSW	2	Fair.
24	7	0	47,5	57	29,80	65		SSW	2	Cloudy.
	2	0	50	57,5	29,69	68		SSW	2	Cloudy.
25	7	0	51	57	29,50	76	0,074	SW	2	Cloudy.
	2	0	54,5	59	29,56	50		SW	2	Cloudy.
26	7	0	47	56,5	29,60	66	0,055	SW	2	Fair.
	2	0	52	58,5	29,58	52		W	2	Cloudy.
27	7	0	44	56	29,48	70	0,103	SSW	2	Cloudy.
	2	0	49,5	58,5	29,49	62		SW	2	Fair.
28	7	0	43	56	29,74	65	0,082	W	1	Fine.
	2	0	52	59	29,84	52		SW	1	Cloudy.
29	7	0	47	57	29,70	60		SSE	2	Fine.
	2	0	56	61	29,61	50		SSE	2	Fine.
30	7	0	44	58	29,66	67	0,140	WSW	1	Cloudy.
	2	0	46	59,5	29,72	65		S	2	Cloudy.
31	7	0	42	57	30,00	65	0,050	W	1	Fair.
	2	0	52	60	29,98	60		SW	2	Cloudy.

METEOROLOGICAL JOURNAL
for April, 1792.

1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 1	7	0	52	57,5	29,70	75	0,039	SW	2	Cloudy.
	2	0	54	60,5	29,65	70		SW	2	Cloudy.
2	7	0	48	58	29,59	66		WSW	2	Cloudy.
	2	0	52,5	60	29,64	64		SW	2	Cloudy.
3	7	0	39,5	57,5	29,66	67	0,048	W	1	Fair.
	2	0	50	58	29,80	51		SW by S	2	Cloudy.
4	7	0	48	57	29,43	69	0,205	W	1	Fine.
	2	0	55	59	29,38	65		WNW	2	Cloudy.
5	7	0	42	57	29,42	65	0,117	WNW	1	Fine.
	2	0	52	58,5	29,50	52		WNW	2	Fair.
6	7	0	43	56,5	29,94	63		NW	1	Cloudy.
	2	0	52	59	30,13	51		NW	2	Fair.
7	7	0	42,5	57,5	30,30	62		WNW	1	Fine.
	2	0	53	60,5	30,32	45		S	2	Fine.
8	7	0	48	57	30,22	51		SSE	1	Hazy.
	2	0	55	61	30,16	45		SE	1	Fair.
9	7	0	47	59	30,07	60		E	1	Fair.
	2	0	57	60,5	30,05	53		E	1	Hazy.
10	7	0	52	60	30,00	61		ENE	1	Fair.
	2	0	60,5	63	29,98	44		E	1	Fine.
11	7	0	49,5	60,5	30,01	61		ENE	1	Fine.
	2	0	63	65,5	30,00	44		E	1	Fine.
12	7	0	52	62	30,05	62		E	1	Fine.
	2	0	62	64	30,02	54		SE	1	Fair.
13	7	0	57	63	29,98	59		E	1	Cloudy.
	2	0	66	64,5	29,98	61		W	1	Fair.
14	7	0	55	63	30,01	60		W	1	Cloudy.
	2	0	61,5	63,5	30,01	59		W	1	Hazy.
15	7	0	52	62	29,98	61		ENE	2	Cloudy.
	2	0	57	61,5	29,94	56		ENE	2	Hazy.
16	7	0	48	60	29,93	61		ENE	2	Fair.
	2	0	56,5	61	29,89	56		ENE	2	Fair.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 17	7	0	52	60	29,70	56		ENE	2	Cloudy.
	2	0	53	59	29,62	62		E	1	Cloudy.
18	7	0	47	58,5	29,26	70	0,251	WNW	1	Rain.
	2	0	48	60	29,12	67		WNW	1	Cloudy.
19	7	0	41	57	29,21	70	0,552	NW	1	Rain.
	2	0	42,5	57	29,33	70		N	2	Rain.
20	7	0	38	56	29,87	66	0,150	N	1	Fine.
	2	0	44,5	55,5	30,07	55		N	2	Cloudy.
21	7	0	42	55	30,23	60		W	2	Cloudy.
	2	0	51,5	57	30,22	50		SW	2	Fair.
22	7	0	48	54,5	30,04	65		SW	2	Cloudy.
	2	0	55	57,5	30,03	59		SW	2	Cloudy.
23	7	0	49	56,5	29,82	59		SW	2	Fine.
	2	0	60,5	59,5	29,77	47		SSW	2	Fine.
24	7	0	51,5	58	29,72	65		NE	1	Cloudy.
	2	0	52,5	59,5	29,73	66		NW	1	Cloudy.
25	7	0	43,5	58	29,89	67	0,188	W	1	Fine.
	2	0	55,5	60	29,96	47		W	1	Fine.
26	7	0	46	58	30,17	66		W	1	Fair.
	2	0	58,5	60,5	30,17	47		W	1	Fine.
27	7	0	52	59	30,15	61		SW	2	Cloudy.
	2	0	57	61,5	30,18	59		SW	2	Cloudy.
28	7	0	54,5	59,5	30,23	65		SW	2	Cloudy.
	2	0	60	60,5	30,28	59		SW	2	Cloudy.
29	7	0	56	60	30,30	63		SW	1	Fair.
	2	0	62	61	30,26	55		NE	1	Fine.
30	7	0	54	60,5	30,01	64		NE	1	Hazy.
	2	0	63	63	29,86	62		NE	1	Fair.

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1792	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M	°	°	Inches.		Inches.	Points.	Str.	
May 1	7	0	45	59,5	29,93	61	0,070	W	1	Fair.
	2	0	53	59,5	29,99	43		WNW	2	Fair.
2	7	0	46	58,5	30,15	56		WNW	1	Fine.
	2	0	53	59	30,18	44		NW	1	Fair.
3	7	0	42,5	57,5	30,36	55		WNW	1	Fine.
	2	0	52	58	30,32	44		WNW	1	Fine.
4	7	0	49	58	30,06	65		WNW	1	Cloudy.
	2	0	53	58	29,88	65		WNW	2	Cloudy.
5	7	0	43	56	30,14	60	0,105	N	2	Fair.
	2	0	50	56,5	30,13	57		NE	2	Cloudy.
6	7	0	45,5	55,5	30,18	62		NE	2	Fine.
	2	0	55	57	30,16	50		NE	2	Fair.
7	7	0	46,5	54,5	30,06	61		NE	2	Fine.
	2	0	57,5	56,5	30,01	45		NE	2	Fair.
8	7	0	46	56	30,06	60		NE	2	Fine.
	2	0	54,5	58	30,02	53		NE	2	Fine.
9	7	0	47	56,5	30,03	60		NE	2	Cloudy.
	2	0	52,5	56,5	30,05	52		NE	2	Cloudy.
10	7	0	44,5	55,5	30,12	58		NE	1	Cloudy.
	2	0	47,5	54,5	30,13	55		NE	1	Cloudy.
11	7	0	44	54	30,17	55		NE	1	Cloudy.
	2	0	50,5	54	30,16	45		NE	1	Cloudy.
12	7	0	45	54	30,16	54		N	1	Cloudy.
	2	0	52,5	54,5	30,18	47		NW	1	Cloudy.
13	7	0	46	54	30,17	56		WNW	1	Fine.
	2	0	56,5	55,5	30,13	46		WSW	1	Cloudy.
14	7	0	50	55	30,06	62		SW	1	Cloudy.
	2	0	52	56	30,06	63		SW	1	Rain.
15	7	0	51	55,5	30,05	67	0,088	SW	1	Cloudy.
	2	0	61,5	58	30,03	51		SW	2	Fair.
16	7	0	53	57	29,88	64	0,030	SW	1	Fair.
	2	0	55	58	29,79	61		SSW	1	Rain.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
May 17	7	0	50	57,5	29,70	72	0,546	SSW	2	Rain.
	2	0	62	59	29,80	50		WSW	2	Fair.
18	7	0	55	58	29,77	72	0,044	WSW	2	Cloudy.
	2	0	63,5	59,5	29,90	52		W	2	Cloudy.
19	7	0	56	59,5	29,91	64	0,038	SW	2	Fair.
	2	0	66,5	62	29,92	50		SW	2	Fine.
20	7	0	57	61	30,04	64		SW	1	Cloudy.
	2	0	67	63	30,01	52		SW	1	Fair.
21	7	0	58	62	29,70	69	0,253	SE	1	Cloudy.
	2	0	61	63	29,84	61		W	1	Rain.
22	7	0	53	62	30,10	62	0,056	W	1	Fair.
	2	0	61	62,5	30,11	61		W	1	Fair.
23	7	0	47	60,5	30,29	58	0,027	NW	1	Fine.
	2	0	56,5	61	30,31	48		NNE	1	Fair.
24	7	0	48	60	30,39	52		E	1	Fine.
	2	0	56,5	60	30,36	51		E	1	Fine.
25	7	0	52	59	30,05	59		ESE	1	Cloudy.
	2	0	59	60	29,90	51		SSE	1	Cloudy.
26	7	0	53	59	29,72	68	0,098	WSW	2	Cloudy.
	2	0	63	61	29,73	51		WSW	2	Fair.
27	7	0	53	60	29,76	65	0,114	WSW	2	Fair.
	2	0	58,5	60	29,62	53		S	2	Cloudy.
28	7	0	51,5	59	29,40	62	0,091	WSW	2	Fair.
	2	0	55	59	29,37	58		S	2	Cloudy.
29	7	0	52,5	58	29,32	62	0,064	S	2	Cloudy.
	2	0	57	59	29,35	56		S	2	Cloudy.
30	7	0	52	58,5	29,62	63		SSW	2	Fine.
	2	0	62,5	60,5	29,60	50		SSW	2	Fair.
31	7	0	52	59,5	29,93	64		W	1	Fine.
	2	0	64	61	29,99	46		W	1	Fine.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
June 1	7	0	53	60,5	30,07	59		S	1	Fine.
	2	0	67	63	30,02	47		SSW	1	Fine.
2	7	0	59	61,5	30,00	67		SW	1	Cloudy.
	2	0	66	63,5	30,18	60		N	1	Fine.
3	7	0	55	61,5	30,24	62		NNE	1	Cloudy.
	2	0	65,5	63	30,24	48		NE	2	Fine.
4	7	0	53	61,5	30,22	59		NE	1	Fair.
	2	0	64	64	30,17	50		NE	2	Fine.
5	7	0	50	61	30,08	65		N	2	Cloudy.
	2	0	62	62	30,02	57		NNE	1	Fair.
6	7	0	51	61	29,93	59		N	1	Cloudy.
	2	0	59,5	61,5	29,93	50		NNE	1	Fair.
7	7	0	52	61	29,91	62		NE	1	Cloudy.
	2	0	60	61	29,87	63		NW	1	Fair.
8	7	0	49	60	29,69	66	0,157	SSE	2	Rain.
	2	0	57	61	29,62	59		S	2	Cloudy.
9	7	0	51	60	29,78	59	0,053	WNW	1	Cloudy.
	2	0	59,5	61	29,76	51		SW	1	Cloudy.
10	7	0	53	60	29,65	60		SW	1	Cloudy.
	2	0	57,5	61	29,55	59		SSW	1	Cloudy.
11	7	0	54	60	29,41	65	0,058	W	1	Fair.
	2	0	61	61	29,48	61		SW	2	Fair.
12	7	0	52	60	29,77	58	0,050	WSW	1	Fine.
	2	0	59	61	29,77	53		W	1	Cloudy.
13	7	0	52	59,5	29,72	62	0,015	SW	1	Cloudy.
	2	0	53	60	29,66	60		SSE	1	Rain.
14	7	0	54	59,5	29,58	69	0,133	WNW	1	Cloudy.
	2	0	59	60,5	29,85	56		NNW	1	Cloudy.
15	7	0	52	60	30,26	60		WNW	1	Fine.
	2	0	66	61,5	30,26	40		WNW	1	Fine.
16	7	0	58	61,5	30,27	56		E	1	Fine.
	2	0	67	63,5	30,26	46		E	1	Fine.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
June 17	7	0	60	62,5	30,01	57		E	1	Fair.
	2	0	72	65	29,96	49		SSW	1	Fair.
18	7	0	55	63,5	29,97	53		WNW	1	Fine.
	2	0	63	64	29,92	49		WNW	1	Fair.
19	7	0	55	63	29,74	57		NW	1	Fair.
	2	0	59	63	29,73	55		NW	2	Cloudy.
20	7	0	50	61,5	29,73	57		NW	1	Cloudy.
	2	0	52,5	62	29,75	66		NW	1	Cloudy.
21	7	0	51,5	60,5	29,80	60	0,072	NW	1	Cloudy.
	2	0	62	61	29,80	47		NW	1	Cloudy.
22	7	0	53	61	29,77	59		W	2	Fair.
	2	0	58,5	61	29,75	57		SW	1	Cloudy.
23	7	0	51	60	29,81	58	0,174	SW	2	Fair.
	2	0	59	60,5	29,80	55		SW	2	Cloudy.
24	7	0	53	60,5	29,88	60	0,051	SSW	2	Cloudy.
	2	0	56	60	29,84	60		S	2	Rain.
25	7	0	52	59,5	30,04	63	0,351	W	1	Cloudy.
	2	0	63	60,5	30,14	51		WNW	1	Cloudy.
26	7	0	55	60,5	30,17	62	0,039	WNW	2	Cloudy.
	2	0	61,5	61	30,18	56		WNW	2	Cloudy.
27	7	0	57	61	30,18	66		SW	1	Cloudy.
	2	0	68	62	30,14	59		SW	1	Cloudy.
28	7	0	58	62	30,18	65		WSW	1	Fair.
	2	0	79,5	64,5	30,15	54		WSW	1	Fair.
29	7	0	63	64,5	30,08	64		W	1	Cloudy.
	2	0	67	65	30,02	64		WSW	1	Rain.
30	7	0	56	64,5	29,88	64	0,471	WNW	1	Fine.
	2	0	63	64,5	29,92	48		WNW	1	Fair.

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1792	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
July 1	7	0	54	63,5	29,99	55		WNW	1	Fair.
	2	0	63	63,5	29,99	46		NW	1	Fair.
2	7	0	54	63,5	30,00	60		WNW	1	Cloudy.
	2	0	64	64	30,00	52		WNW	1	Cloudy.
3	7	0	56,5	63,5	29,96	57		WNW	1	Cloudy.
	2	0	60,5	64	29,91	53		WNW	1	Cloudy.
4	7	0	57	63	29,80	67		SW	2	Cloudy.
	2	0	64	64	29,80	62		SW	1	Cloudy.
5	7	0	57	64	29,95	61		W	1	Fair.
	2	0	59,5	64	29,85	59		SW	2	Cloudy.
6	7	0	57	64	30,00	57		WNW	1	Fair.
	2	0	60	64	30,02	55		WNW	1	Cloudy.
7	7	0	58	64	30,07	67		WNW	1	Cloudy.
	2	0	72	65	30,02	56		WSW	1	Fair.
8	7	0	58	65	29,94	61		SW	2	Cloudy.
	2	0	72	65	29,96	54		SW	1	Cloudy.
9	7	0	57	65	30,01	64		SW	1	Cloudy.
	2	0	65	65	30,01	61		SSW	1	Cloudy.
10	7	0	61	65	29,93	78	0,296	E	1	Cloudy.
	2	0	68	66	29,94	63		SE	1	Fair.
11	7	0	60	65,5	29,92	76	0,054	NW	1	Cloudy.
	2	0	64	66	29,84	75		NNE	1	Cloudy.
12	7	0	57	65	29,60	76	0,128	NNE	1	Cloudy.
	2	0	61	65	29,59	74		NNE	1	Cloudy.
13	7	0	57,5	65	29,68	79	0,067	WNW	1	Rain.
	2	0	62	64,5	29,78	76		W	1	Cloudy.
14	7	0	58	64	30,03	71	0,048	W	1	Cloudy.
	2	0	69	66	30,10	50		W	1	Cloudy.
15	7	0	60	65	30,19	63		W	1	Fair.
	2	0	75,5	66	30,19	50		W	1	Fair.
16	7	0	63	65,5	30,07	59		SW	1	Fine.
	2	0	76	69	29,96	50		SSE	1	Fine.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
July 17	7	0	65	67,5	29,69	69	0,220	SW	2	Cloudy.
	2	0	73	68	29,76	51		SW	2	Fair.
18	7	0	58	67	29,77	69		SW	2	Cloudy.
	2	0	71,5	68	29,79.	48		SW	2	Fine.
19	7	0	60	66	29,93	62		SW	2	Fair.
	2	0	71,5	67	29,93	49		W	2	Fair.
20	7	0	58	66	29,95	64		W	1	Fair.
	2	0	67	67	29,87	50		E	1	Cloudy.
21	7	0	55	65	29,52	80	0,510	W	2	Cloudy.
	2	0	56	64,5	29,62	79		WNW	1	Rain.
22	7	0	54	64	29,97	66	0,601	WNW	1	Fair.
	2	0	65	64	30,05	60		WNW	1	Fair.
23	7	0	55	63	30,05	58		WNW	1	Cloudy.
	2	0	65	64	30,00	49		E	1	Cloudy.
24	7	0	53	63	29,95	60		E	1	Fair.
	2	0	68	64	29,92	48		E	1	Cloudy.
25	7	0	55	63	29,86	64		SE	1	Cloudy.
	2	0	60	64	29,77	63		SSE	2	Cloudy.
26	7	0	58	63	29,70	68	0,017	SW	1	Fine.
	2	0	71,5	64,5	29,70	47		SW	1	Fair.
27	7	0	58	63,5	29,58	67	0,061	SW	2	Cloudy.
	2	0	64,5	64	29,60	53		SW	2	Cloudy.
28	7	0	55	63,5	29,69	61		SW	1	Fine.
	2	0	71	65	29,71	46		SW	1	Fair.
29	7	0	57	63,5	29,61	67		SSW	1	Rain.
	2	0	60	64	29,52	65		E	1	Cloudy.
30	7	0	55	63	29,87	68	0,261	N	1	Fair.
	2	0	69	65	29,96	51		NW	1	Fair.
31	7	0	55,5	64	30,10	65	0,036	WSW	2	Cloudy.
	2	0	62	64	30,12	63		SSW	2	Cloudy.

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1792	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.*
	H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Aug. 1	7	0	62	64	30,30	68		S	1	Cloudy.
	2	0	71,5	68	30,26	53		E	1	Fine.
2	7	0	60	66	30,15	73		E	1	Cloudy.
	2	0	76,5	68,5	30,08	53		E	1	Fine.
3	7	0	65	68	30,00	65		E	1	Fine.
	2	0	76	71,5	29,96	51		E	2	Fine.
4	7	0	63	69	29,99	64		E	1	Fine.
	2	0	75	71,5	30,04	50		E	1	Fine.
5	7	0	60	69	30,17	64		ENE	1	Cloudy.
	2	0	72,5	70,5	30,12	50		NE	1	Fair.
6	7	0	62	69	30,11	67		ENE	1	Cloudy.
	2	0	73	71	30,06	46		E	1	Fine.
7	7	0	61	69	30,06	61		NE	1	Cloudy.
	2	0	72	71	30,11	50		NE	1	Fine.
8	7	0	60	69	30,20	68		E	1	Cloudy.
	2	0	73	70	30,22	53		E	1	Fair.
9	7	0	60	68	30,20	65		E	1	Hazy.
	2	0	78	72	30,17	49		SSE	2	Hazy.
10	7	0	64	69	30,18	59		SSE	2	Cloudy.
	2	0	79	73	30,17	46		SSE	1	Fair.
11	7	0	63	70	30,16	60		SSE	1	Fair.
	2	0	82	73	30,11	45		SSW	1	Fine.
12	7	0	68	70	30,10	53		SW	1	Cloudy.
	2	0	84	74	30,05	45		SW	1	Hazy.
13	7	0	65	72	30,00	62		SW	1	Fine.
	2	0	78	74	29,97	48		SW	1	Fine.
14	7	0	65	72	29,98	55		W	1	Fair.
	2	0	75	73	29,98	48		WNW	1	Fair.
15	7	0	61	71	30,04	58		NW	1	Cloudy.
	2	0	71,5	72	30,04	48		WNW	1	Fine.
16	7	0	61	71	29,85	69	0,031	WNW	1	Cloudy.
	2	0	73	71	29,80	56		WNW	1	Cloudy.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Aug. 17	7	0	60	71	29.80	63		W	1	Cloudy.
	2	0	73	71	29,79	49		W	1	Cloudy.
18	7	0	59	70	29,52	80	0,400	NE	1	Rain.
	2	0	61	69	29,55	76		NE	1	Rain.
19	7	0	55	68	29,76	69	0,885	NNW	1	Fine.
	2	0	67	68	29,78	51		NW	1	Cloudy.
20	7	0	54	66	29,93	65		NW	1	Fair.
	2	0	65	66	29,90	53		NW	1	Cloudy.
21	7	0	56	65	29,59	77	0,440	SW	2	Rain.
	2	0	70	68	29,53	61		SW	2	Fair.
22	7	0	57	66	29,47	66	0,041	SW	2	Rain.
	2	0	68	67	29,40	53		SW	2	Fair.
23	7	0	57	65	29,41	63		SW	2	Fair.
	2	0	67	66	29,47	53		W	2	Cloudy.
24	7	0	58	65	29,84	68	0,028	W	2	Fair.
	2	0	70	66	29,94	51		W	2	Fair.
25	7	0	58	65	29,93	67	0,075	SW	2	Rain.
	2	0	66,5	66,5	29,87	61		SW	2	Cloudy.
26	7	0	62	66	29,71	72		SW	1	Cloudy.
	2	0	64	67	29,60	70		SSE	2	Rain.
27	7	0	57	66	29,70	63	0,165	WSW	2	Fine.
	2	0	69,5	67	29,80	47		WSW	2	Fine.
28	7	0	54	65	29,91	66		W	1	Fine.
	2	0	68,5	67	29,94	47		ENE	1	Fine.
29	7	0	57	66	30,12	62		NE	1	Cloudy.
	2	0	64	65,5	30,12	55		NE	2	Cloudy.
30	7	0	58	65	29,95	62		NE	1	Cloudy.
	2	0	62	65	29,92	66		E	1	Cloudy.
31	7	0	57	65	30,00	71		E	1	Cloudy.
	2	0	68	65,5	29,98	59		ESE	2	Cloudy.

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1792	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Sept. 1	7	0	60	65	29,88	69	0,125	SE	1	Cloudy.
	2	0	62	65,5	29,88	68		SSE	1	Cloudy.
2	7	0	59	65	29,90	71	0,068	SSE	1	Fine.
	2	0	69	66	29,90	55		SSE	2	Fair.
3	7	0	57	65	29,81	70		SSE	1	Cloudy.
	2	0	66,5	65,5	29,77	60		SSW	1	Fair.
4	7	0	54	64,5	29,60	67	0,052	WSW	1	Fair.
	2	0	67,5	65	29,60	56		SW	1	Cloudy.
5	7	0	49	64	29,74	70		SW	1	Foggy.
	2	0	66,5	65	29,84	54		WSW	1	Fair.
6	7	0	54	64	30,13	70		NNE	1	Foggy.
	2	0	67,5	65	30,15	58		SSE	1	Fair.
7	7	0	57	64	30,14	67		WSW	1	Cloudy.
	2	0	65	64,5	30,08	62		WSW	1	Cloudy.
8	7	0	60	64	29,95	72		WSW	1	Cloudy.
	2	0	65	65	29,95	61		WSW	1	Cloudy.
9	7	0	53	63	30,00	67		NW	1	Cloudy.
	2	0	61	64	29,98	58		W	1	Cloudy.
10	7	0	60	63,5	29,70	66		WSW	2	Cloudy.
	2	0	65	65	29,76	48		WNW	2	Fair.
11	7	0	49	63	29,81	62		WNW	2	Fine.
	2	0	57	63	29,90	57		WNW	1	Fair.
12	7	0	49,5	62	30,08	63	0,030	WNW	1	Cloudy.
	2	0	56	62	29,84	67		WSW	2	Rain.
13	7	0	53	61	29,62	62	0,172	WSW	2	Fine.
	2	0	63	62	29,70	51		WNW	2	Fair.
14	7	0	51	61	29,89	68		WNW	1	Cloudy.
	2	0	57	61	29,74	74		WNW	1	Rain.
15	7	0	45	59,5	30,03	70	0,493	NW	2	Cloudy.
	2	0	53	60	30,19	51		NNW	2	Fine.
16	7	0	43	59	30,34	64		N	1	Fine.
	2	0	56,5	59,5	30,34	55		WNW	1	Fine.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Sep. 17	7	0	48	59	30,17	61		W	1	Cloudy.
	2	0	57	59	30,11	57		SSW	1	Cloudy.
18	7	0	47,5	58	30,01	71	0,030	SSW	1	Cloudy.
	2	0	60	59	29,95	59		SSW	1	Cloudy.
19	7	0	57	59	29,87	74		SW	2	Cloudy.
	2	0	60	60	29,78	67		SSW	2	Cloudy.
20	7	0	48	59	29,52	73	0,213	W	1	Fine.
	2	0	52	59,5	29,40	65		WSW	2	Rain.
21	7	0	43	57,5	29,38	69	0,280	WSW	2	Fine.
	2	0	52	58	29,24	63		WSW	2	Rain.
22	7	0	44	56,5	29,09	68	0,072	W	2	Fine.
	2	0	54	58	29,24	60		WNW	2	Rain.
23	7	0	42	56	29,47	70	0,043	NW	1	Fine.
	2	0	53	57	29,57	63		WNW	1	Cloudy.
24	7	0	47	55,5	29,62	69	0,017	W	2	Rain.
	2	0	54	57	29,45	70		S	2	Cloudy.
25	7	0	48	55,5	29,33	66	0,014	W	2	Cloudy.
	2	0	54	56	29,54	65		NW	2	Cloudy.
26	7	0	45	55	30,02	70	0,023	WNW	1	Fine.
	2	0	56	57	30,06	55		NW	1	Fair.
27	7	0	47	55,5	30,07	65		NNW	1	Cloudy.
	2	0	57	56,5	30,00	56		NNW	1	Cloudy.
28	7	0	54	56	29,73	78	0,040	SW	2	Cloudy.
	2	0	56,5	57	29,58	72		SSW	2	Cloudy.
29	7	0	50	56,5	29,47	74	0,071	SSW	1	Fine.
	2	0	53	57	29,44	70		S	2	Rain.
30	7	0	56	57,5	29,40	81	0,167	E	1	Cloudy.
	2	0	62	58	29,49	67		E	1	Cloudy.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Oct. 1	7	0	56	58	29,62	78	0,020	E	1	Fair.
	2	0	63	60	29,63	68		E	1	Cloudy.
2	7	0	56	59	29,73	80	0,043	ENE	1	Cloudy.
	2	0	57	59,5	29,70	70		NE	1	Cloudy.
3	7	0	53	59	29,80	67		ENE	2	Cloudy.
	2	0	52	58	29,83	65		ENE	2	Cloudy.
4	7	0	47	57	29,88	64		ENE	1	Cloudy.
	2	0	49	56,5	29,82	62		ENE	1	Cloudy.
5	7	0	47	57	29,74	67		E	1	Cloudy.
	2	0	50	58	29,73	63		ENE	1	Cloudy.
6	7	0	49	57	29,71	73		ENE	1	Cloudy.
	2	0	52	59	29,72	70		NE	2	Cloudy.
7	7	0	48,5	57	29,78	73	0,028	NNE	1	Cloudy.
	2	0	52	59	29,80	63		E	1	Cloudy.
8	7	0	47,5	56,5	29,85	64		NE	1	Cloudy.
	2	0	52	58,5	29,85	59		NE	1	Cloudy.
9	7	0	46,5	56,5	29,80	64		NE	1	Cloudy.
	2	0	52	58	29,78	60		NNE	1	Cloudy.
10	7	0	46	56,5	29,76	77	0,062	N	1	Rain.
	2	0	49	58	29,76	79		N	1	Rain.
11	7	0	43	57	29,83	74	0,507	NW	1	Fine.
	2	0	53	58	29,87	62		NW	1	Fair.
12	7	0	40	56	29,76	73		WNW	1	Cloudy.
	2	0	53	58	29,59	62		S	2	Cloudy.
13	7	0	44	56	29,46	68		SSW	1	Fair.
	2	0	54	58	29,39	68		SSE	2	Cloudy.
14	7	0	52	56,5	29,26	81	0,383	S	2	Rain.
	2	0	52,5	59	29,29	68		SW	2	Cloudy.
15	7	0	52	58	29,38	76	0,168	SSW	2	Rain.
	2	0	55	59,5	29,36	69		SSW	2	Cloudy.
16	7	0	48	58	29,68	71	0,135	SW	1	Cloudy.
	2	0	53	59	29,63	67		SW	2	Cloudy.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Oct. 17	7	0	46	58	29,78	73	0,130	WSW	1	Fair.
	2	0	55	60	29,76	67		SW	2	Fair.
18	7	0	43	58	29,67	69		WSW	2	Fine.
	2	0	53	60	29,77	58		W	1	Fair.
19	7	0	39	57	29,95	71		W	1	Fair.
	2	0	50	58,5	29,95	58		W	1	Hazy.
20	7	0	53	58	29,71	77		S	2	Cloudy.
	2	0	57	59	29,65	72		SSW	2	Cloudy.
21	7	0	51	58,5	29,58	81	0,025	S	1	Fine.
	2	0	56	60	29,54	74		SSE	2	Cloudy.
22	7	0	52	59	29,68	76	0,068	SSE	2	Fair.
	2	0	58,5	61,5	29,78	64		SSW	2	Fair.
23	7	0	48	59,5	30,05	71		SSW	2	Fine.
	2	0	56,5	61	30,11	66		SSW	1	Cloudy.
24	7	0	42	59	30,37	68		WSW	1	Cloudy.
	2	0	51	59,5	30,42	63		NE	1	Cloudy.
25	7	0	42	57,5	30,32	67		NE	1	Cloudy.
	2	0	50	58,5	30,30	67		NE	1	Cloudy.
26	7	0	44	56	30,12	66		ENE	1	Cloudy.
	2	0	47,5	57,5	30,00	63		E	2	Cloudy.
27	7	0	48	56	29,93	74		E	1	Cloudy.
	2	0	58	59	29,96	71		ESE	1	Cloudy.
28	7	0	54	57,5	29,90	83		SE	1	Cloudy.
	2	0	58	60	29,92	73		SSE	1	Fair.
29	7	0	52	58	29,90	84		W	1	Cloudy.
	2	0	58	60	29,93	71		W	1	Cloudy.
30	7	0	48	58	29,95	77		W	1	Cloudy.
	2	0	54,5	59,5	29,82	77		SW	2	Cloudy.
31	7	0	53	59	29,48	80	0,315	WSW	1	Cloudy.
	2	0	57	61	29,61	60		W	1	Fair.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Nov. 1	7	0	48	59	29,82	70	0,035	W	2	Fair.
	2	0	52	59,5	29,75	70		SSW	2	Cloudy.
2	7	0	42	58	30,00	68	0,080	WNW	1	Fine.
	2	0	50,5	61	30,14	63		WNW	1	Fine.
3	7	0	59	59	30,19	84		W	1	Cloudy.
	2	0	58	62	30,20	63		SW	1	Fine.
4	7	0	47	60	30,16	75		SW	1	Fine.
	2	0	55,5	63,5	30,17	69		SSW	1	Fine.
5	7	0	42	60	30,22	73		SSW	1	Foggy.
	2	0	50	60,5	30,25	77		SSW	1	Foggy.
6	7	0	39	58	30,25	77				Foggy.
	2	0	46	59,5	30,24	79				Foggy.
7	7	0	46	58,5	30,24	75		SSW	1	Cloudy.
	2	0	49,5	58,5	30,27	70		SSW	1	Cloudy.
8	7	0	49	58,5	30,41	70		SSW	1	Cloudy.
	2	0	52	59	30,44	64		SSW	1	Cloudy.
9	7	0	49	59	30,40	67		SSW	1	Cloudy.
	2	0	53	59	30,32	68		SSW	1	Cloudy.
10	7	0	49,5	58	30,24	71		SSW	1	Cloudy.
	2	0	53	59,5	30,24	66		SSW	1	Cloudy.
11	7	0	49	59	30,22	76		SSW	1	Cloudy.
	2	0	50	60	30,17	70		SSW	1	Cloudy.
12	7	0	42	57	29,92	70		E	1	Cloudy.
	2	0	50	60	29,78	67		ESE	1	Fine.
13	7	0	48	58	29,47	73		E	1	Cloudy.
	2	0	52	59,5	29,44	65		S	2	Cloudy.
14	7	0	46	57	29,32	71	0,066	S	2	Rain.
	2	0	52	58,5	29,36	63		SSW	2	Cloudy.
15	7	0	44	57,5	29,64	72		WSW	2	Cloudy.
	2	0	49	58	29,73	59		W	1	Fair.
16	7	0	39	56	29,88	66		WNW	1	Fine.
	2	0	46	57,5	29,94	64		WNW	1	Fair.

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1792	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Nov. 17	7	0	34	54,5	30,10	67		NW	1	Fine.
	2	0	40	57	30,18	63		NW	1	Fine.
18	7	0	33	53,5	30,27	67		NW	1	Fair.
	2	0	44	56,5	30,16	65		W	1	Cloudy.
19	7	0	48	54	29,88	67		W	1	Cloudy.
	2	0	50	57	29,95	60		W	1	Fair.
20	7	0	35	55	30,42	64		NW	1	Foggy.
	2	0	40	55	30,43	63		WNW	1	Cloudy.
21	7	0	48	54	30,00	78		WSW	2	Cloudy.
	2	0	52	55,5	29,78	73		SW	2	Cloudy.
22	7	0	41	54	29,64	68	0,183	WNW	1	Fine.
	2	0	46	55,5	29,60	63		NW	2	Fine.
23	7	0	38	54,5	29,80	66		NNW	2	Fine.
	2	0	45	55	29,83	68		NNW	2	Cloudy.
24	7	0	39	54	30,26	72	0,090	NE	1	Cloudy.
	2	0	46	57	30,27	66		NE	1	Cloudy.
25	7	0	41	53	30,24	65		NE	1	Fine.
	2	0	47	55,5	30,14	66		NE	2	Cloudy.
26	7	0	39	53,5	29,93	70		NE	1	Cloudy.
	2	0	43,5	55,5	29,84	71		NE	1	Cloudy.
27	7	0	42	53	29,83	71		NE	1	Cloudy.
	2	0	45	54,5	29,83	66		NE	1	Cloudy.
28	7	0	39	53	29,95	72		E	1	Foggy.
	2	0	41	55	29,92	73		E	1	Cloudy.
29	7	0	38	52	29,91	68		E	1	Fine.
	2	0	43	54	29,95	66		E	1	Fine.
30	7	0	38	52	30,05	72		E	1	Cloudy.
	2	0	37,5	53	30,00	70		ESE	1	Cloudy.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Dec. 1	8	0	34	50	29,85	70		E	1	Cloudy.
	2	0	34	51	29,85	69		E	1	Cloudy.
2	8	0	37	50	30,15	69		E	1	Cloudy.
	2	0	38,5	51,5	30,21	69		NE	1	Cloudy.
3	8	0	37	50	30,35	66		E	1	Foggy.
	2	0	35	50,5	30,34	66		W	1	Cloudy.
4	8	0	37	49,5	30,26	78		WNW	1	Foggy.
	2	0	42	51,5	30,11	80		WSW	1	Cloudy.
5	8	0	52	53	29,60	79	0,152	WSW	1	Cloudy.
	2	0	53	55,5	29,56	77		WSW	1	Cloudy.
6	8	0	43	53,5	29,64	73	0,041	SSW	1	Cloudy.
	2	0	50,5	56,5	29,34	70		SW	2	Cloudy.
7	8	0	38,5	53,5	29,67	62	0,097	WNW	2	Fair.
	2	0	39,5	56	29,92	59		NW	2	Fine.
8	8	0	32	52	30,25	73		NW	1	Fine.
	2	0	40	55	30,30	66		NW	1	Fine.
9	8	0	45	53	30,09	66	-	W	2	Cloudy.
	2	0	49	54,5	29,91	64		W	2	Cloudy.
10	8	0	52	55	29,88	72		W	2	Cloudy.
	2	0	53,5	57	29,85	68		W	2	Cloudy.
11	8	0	44	55	30,00	63		W	2	Cloudy.
	2	0	45,5	57	30,00	60		WNW	2	Fair.
12	8	0	37	54	30,27	62		WNW	1	Fair.
	2	0	42	55,5	30,25	63		NW	1	Cloudy.
13	8	0	38	53	29,68	78	0,302	ENE	1	Cloudy.
	2	0	39	55	29,72	75		NE	1	Cloudy.
14	8	0	49	54	29,76	70	0,162	WNW	1	Cloudy.
	2	0	50	57	29,80	65		WNW	1	Cloudy.
15	8	0	45	55	29,90	68		NW	1	Fair.
	2	0	49	58	29,93	65		NW	1	Fair.
16	8	0	39	55	30,22	73		ENE	1	Cloudy.
	2	0	43	57	30,24	68		ENE	1	Cloudy.

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1792	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Dec. 17	8	0	42	54	30,17	78		S	1	Cloudy.
	2	0	48	57	30,10	73		SW	2	Cloudy.
18	8	0	48	56	29,85	76		WSW	2	Cloudy.
	2	0	51,5	59	29,82	76		W	2	Cloudy.
19	8	0	49	57,5	29,80	67		WNW	2	Fair.
	2	0	51	59	30,00	58		WNW	2	Fine.
20	8	0	52	58	29,73	78		SW	2	Cloudy.
	2	0	53	60	29,64	74		W	2	Cloudy.
21	8	0	38	56	29,71	66	0,103	NW	2	Fine.
	2	0	43	59	29,87	61		NW	2	Fine.
22	8	0	45	55	29,58	74	0,051	SW	2	Cloudy.
	2	0	45	57	29,22	67		SW	2	Fair.
23	8	0	34,5	54	29,45	60	0,095	N	2	Fine.
	2	0	33	56	29,58	55		N	2	Fine.
24	8	0	31,5	51,5	29,78	63		NNW	2	Fair.
	2	0	34,5	54	29,79	63		NNW	2	Fair.
25	8	0	32	51	29,54	75		NW	1	Foggy.
	2	0	38	52	29,51	72		WNW	1	Cloudy.
26	8	0	37	51	29,00	77	0,348	WNW	1	Cloudy.
	2	0	36	53	29,00	77		WNW	1	Snow.
27	8	0	36	51	29,48	77	0,177	WNW	1	Cloudy.
	2	0	39	53	29,56	70		NW	1	Cloudy.
28	8	0	36,5	50	29,61	72	0,020	NW	1	Fair.
	2	0	41	53	29,71	67		NW	1	Fine.
29	8	0	41	51	29,88	79	0,103	WSW	1	Cloudy.
	2	0	47	53	29,85	81		WSW	1	Cloudy.
30	8	0	44	52	29,86	77	0,115	W	1	Cloudy.
	2	0	45	55,5	29,95	68		WNW	1	Fine.
31	8	0	32,5	51	30,27	72		NW	1	Cloudy.
	2	0	33,5	54	30,27	73		WNW	1	Hazy.

1792.	Thermometer without.			Thermometer within.			Barometer.			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.				Inches.
January	53	19	37,2	57,5	43	51,1	30,47	28,94	29,66				1,810
February	56	16,5	40,0	60	43	52,6	30,40	29,53	29,98				0,712
March	56	26	44,3	61	46	54,3	30,51	29,07	29,77	85	48	64,3	1,791
April	66	38	52,0	65,5	54,5	59,3	30,32	29,12	29,90	75	44	59,6	1,550
May	67	42,5	53,3	63	54	58,2	30,39	29,32	29,97	72	43	56,7	1,624
June	79,5	49	58,1	65	59,5	61,6	30,27	29,41	29,93	67	40	57,7	1,624
July	76	53	61,7	69	63	64,8	30,19	29,52	29,88	80	46	60,9	2,299
August	84	54	65,7	74	64	68,5	30,30	29,40	29,93	80	45	59,1	2,065
September	69	42	55,1	65,5	55	60,5	30,34	29,09	29,79	81	48	60,5	1,910
October	63	39	51,0	61,5	56	58,3	30,42	29,26	29,79	84	58	69,8	1,884
November	58	35	45,5	63,5	52	56,9	30,44	29,32	30,02	84	59	68,9	0,454
December	53	31,5	41,9	60	49,5	54,2	30,35	29,00	29,85	81	58	69,9	1,766
Whole year			50,5			58,4			29,87				19,489

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCIII.

PART II.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCIII.

CONTENTS.

PHILOSOPHICAL

TRANSACTIONS.

XII. *A Description of a Transit Circle, for determining the Place of celestial Objects as they pass the Meridian. By the Rev.* Francis Wollaston, *LL. B. and F. R. S.*

Read May 9, 1793.

An instrument which, in one observation, is capable of giving with precision both the right ascension and declination of celestial objects, has always appeared to me one of the desiderata in astronomy. Though I had often considered the various methods practiced for ascertaining each, and turned it in my mind how I could contrive to make one instrument answer both purposes; I never could satisfy myself in what way to effect the one, without destroying the accuracy of the other; till one evening, at a meeting of our Society in the beginning of 1787, Mr. Ramsden mentioned to me his idea of reading off the divisions of an instrument, by a microscope having a micrometer in the field of view, which, being detached from the limb, could examine with accuracy the distance of the nearest division from a fixed point. It occurred to me immediately, that this was the thing I wanted: because a circle attached to the telescope of a transit instrument, and passing in review before such a microscope, or a pair of such microscopes, would answer the purpose. I did not then know, that a microscope of that kind had been applied by the late Duc de CHAULNES, to his dividing engine, for determining the divisions; described minutely by him, and published in 1768; a copy of which is in our library. Neither did I then know of the same idea having been the foundation of ROËMER's method of reading off the divisions on his *Circulus meridionalis;* an account of which was published by HORREBOW, in the beginning of this century; where a reticule of ten squares was made, by trials of its distance from the limb of the instrument, to coincide with a division of ten minutes on that limb. With them I was not acquainted, till after my instrument was already in some forwardness. Whether Mr. RAMSDEN took the first hint from either of them, and improved upon it, I cannot say. He has brought it into use among us: I certainly derived it from him; and to him I acknowledge myself indebted for it.

This method of reading off has, indeed, been applied already with great success to different instruments; but I do not know that it has ever yet been adapted to the transit. Circles of various kinds have been constructed with wonderful accuracy, yet all have been formed with another view; and their turning freely in azimuth, seemed to render them less fit for the purpose which I wanted; *i. e.* a circle, firmly fixed, and turning truly in the plane of the meridian by means of a transverse axis; with all the adjustments of a transit at the end of the axis itself (which appear to me essential to a due performance); and at the same time with the opposite readings, and all the adjustments of the circles now in use.

On this idea the following instrument was constructed: and since there are some particular contrivances in it, which are new, I take the liberty of laying a general description of it before this Society; not by way of setting forth the praises of any one instrument; but that this may be known in all its parts, with the advantages and disadvantages of each, as far as I have discovered any; and that such of them as are judged useful, (if the Committee shall think proper to honour this paper with a place in our Transactions) may be adopted by others.

My first design was, not to have given orders for one myself, but merely to communicate the thought to those who might improve upon it. Accordingly, I mentioned it first to Mr. Ramsden, in 1788: but the multiplicity of his engagements, and the fertility of his own imagination, (at that time particularly turned towards contriving to make instruments move freely in azimuth) rendered him disinclined to listen to a scheme for one on another plan. The same was the case with Mr. Troughton. I mentioned it likewise to several of my acquaintance: but (perhaps the trouble and expense attending the construction of a first instrument, seldom so complete as a second or a third of the kind, might be the cause) no one was set about. After three years waiting, and becoming, in the mean time, more and more convinced of the advantages of such an instrument to astronomy; and Mr. Cary being recommended to me, as fully qualified for the purpose; though I am growing too old to expect to make many more observations, I gave orders for one of a size and form which I thought most convenient to myself. Observers know best what it is they want; and an instrument-maker who will condescend to listen to them, is a treasure. In this, as well as other respects, it is but justice to Mr. CARY to say, that he has answered the character which was given of him. He has shewn himself, during the whole time, very diligent and attentive; comprehending readily my directions; giving freely his opinion, and his reasons for dissent, if he disapproved of what was proposed; yet being willing to follow mine, if I still continued in the same mind; improving upon some of my hints; and executing in a masterly way every part of it.

The drawing accompanying this paper (Tab. XV.) will shew the general form of the instrument; and need very little explanation.

The whole stands on three feet, adjustable by screws. The bottom plate (of $21\frac{3}{4}$ inches diameter) turns in azimuth; not on a long axis, but on a center; and rides on a bell-metal circle, truly turned, and to which the bottom plate itself is ground. In this way it moves very smooth by hand; but it is capable of being turned by a winch, with tooth and pinion. The intent of its turning thus, is merely for the convenience of reversing the instrument: for, though it might be used out of the meridian, and for azimuths; yet, since it is designed principally for meridian passages, when it is in its place the whole is clamped firmly to the bottom frame by four clamps, which confine it to the circle on which it rides: and this method of turning proves itself to be steady, by the levels on the bottom plate never altering in the least upon screwing the clamps.

The four pillars, and their braces, explain themselves. They stand over the bell-metal circle; and the clamps are placed near the foot of each, for greater steadiness; since they carry the Y*s* for the pivots of the transit.

The construction of these Y*s* is peculiar: they hang, as it were, in gimmals, though of a very firm kind, a drawing of which (Tab. XVI. fig. 1) will best explain them. They have a horizontal motion, smooth and steady: the T, or frame, AB, which carries them, turning on a perpendicular axis of 2½ inches, CD, ground to its socket; on the outside of the plate EF, which connects them with the pillars, and resting on that plate, to which the bottom of the frame itself is ground likewise. In this frame they have a vertical motion: the Y*s* themselves carrying a horizontal axis at AB, which, consisting of two frusta of cones on each side, in contrary directions, with a collar over them, guards against any shake whatsoever, while it admits of the Y adapting itself to the direction of the pivot. The idea of hanging them in this way, as well as that of turning the whole instrument in azimuth on a ground plate, was suggested by our late member Mr. JOHN SMEATON; to whom the world has, during many years, been indebted for repeated capital improvements in mechanics.

By thus hanging the Y*s*, the pivots have a bearing on them from end to end; instead of riding on a bell-metal *ridge*, as is the usual method where the Y*s* are fixed, and cannot set themselves in the direction of the axis. This seems to be a better bearing, and much less likely to wear the pivots.

Yet, to guard against any wear, a pair of cylindrical springs, included in a tube, are applied through rings within the connecting plate above mentioned. These carry, each of them, a pair of rollers, on which a brass collar at each end of the axis of the telescope rides. The springs may be used or removed, at pleasure; and they can be strengthened or weakened, by means of a screw at the bottom of the tube, so as to take off from the pivots any part of the weight that may be judged best: and, since they are in a line with the axis, and are made capable of obeying it in every direction, there is no danger of their deranging its adjustments, while they render its motion exceedingly light and smooth indeed.

The adjustments of the Y*s* are both of them at the same end of the axis, opposite to the divided circle and the microscopes; because the smallest adjustment of that end of the axis between the microscopes, would have affected them so as to require an entire re-adjustment of them too. At the farther end, the axis is perforated, to admit light for illuminating the wires. And I find that (to my sight at least) it is much most agreeable to close the aperture with a pale green glass. The axis itself is 18 inches long, exclusive of the pivots, which are about $1\frac{1}{4}$ each.

The microscopes need no description. They are on the same principle as those described by our late member Major General ROY. (Phil. Trans. Vol. LXXX, p. 145.) Mine are 9 inches long; the object-end at 2 inches from the limb of the circle. They magnify 24 times. One revolution of the micrometer-screw is equal to one minute; and the head is divided to seconds.

The fixed or stationary wire in them, is at the first notch, or minute itself; and it is adjusted by means of a plumb-line, which hangs from the top plate, and passes by the side of the axis; at about 8 degrees, or 1 inch $\frac{3}{4}$, from the centre. For this purpose there are dots made on the limb, at a suitable distance on each side of the zero, both above and below, whether the telescope be horizontal or perpendicular either way. These are viewed through two compound microscopes, (of $5\frac{1}{2}$ inches long, and their object-glass at 3 inches distance from the limb) carried by the same frames as the other microscopes.

The cursor, or moveable wire, in the micrometer-microscopes, is adjusted much in the same way as General ROY's; excepting that the micrometer head is made to turn stiffly on the neck of the screw, so as to allow of bringing the point of zero to front the eye, without the trouble of re-adjustment, if it happened to fall behind.

It may be asked, since I use a compound microscope for viewing the wire, why I choose a plain plumb-line close to the limb, in preference to one in the combined focus of the glasses? My reason is this; I use a compound microscope, because my eyes do never, with any comfort, adapt themselves to a single magnifier; and in this way I have more light, and can keep my eye at a greater distance. I approve much of Mr. RAMSDEN's ghost, as it is called, where it can be used with safety. But in this instrument, I thought I could not confide in it, as the microscope must be on a different support: whereas the looking at the dot itself, and the wire together, through a tube of above 5 inches, and at a distance of 10 or 12 from the limb, could admit of little or no parallax. I had intended making use of the original dots of the divisions for this purpose; but they are so minute, that the smallest wire capable of supporting a plummet eclipses them entirely.

There is of course a level (and a very sensible one it is) for adjusting the axis. The circle was ordered to have ten radii; that when the telescope is horizontal, and pointing to a meridian mark, there might be a vacancy between the cones, above or below, for introducing a level. In the brace between the pillars, over the moveable Y, (at A, Tab. XV.) it may be observed, the bottom bar is omitted; in order to give the better room for passing the level, without inclining it, or running any hazard of striking it. From the lower bar of the opposite brace B, over the fixed Y, there stands out a forked piece of brass, to receive the leg of the level, and direct it to its place; as also for keeping it upright when the foot stands on the pivot, and just allowing a very little shake, so as not to cramp it. By this contrivance the level is easily handled, and reversed, without danger of disturbing it or the instrument.

The top plate, as may be seen in the drawing, has a large opening cut more than half way across it. The design of this is, to allow you to observe quite up to the zenith, and a little beyond it, clear of all obstruction whatsoever. And since the whole instrument is capable of being reversed, or turned half way round in azimuth; when you have occasion to observe the transit of stars, in that part of the heavens where they would be intercepted by the plate in one position, it is entirely out of the way in the other.

The circle itself is of full two feet diameter at the divisions; being $25\frac{1}{2}$ inches at the edge. The undivided circle, on the side of the telescope next to the open end of the axis, serves for strength and uniformity; and to it is applied the clamp for elevation. That clamp is so made, as to allow the circle to run freely all round if you please; not bearing at all against it, but supporting itself, and yet being easily removeable. It has no command over the circle whatever, when handled with care, excepting in the altitude of the telescope, by an adjusting screw when the clamp is set: and, as that screw has a milled head at each end, it is as conveniently turned from the one as from the other side of the instrument, to bring the horizontal wire to bisect the object.

The telescope is of 2 inches aperture, and 33 focal length. The object-glass does not slide within the tube; but screws into the end of a piece of false tube, of 4 inches length, which slides on the outside of the principal tube, and is fixed in its place, by 3 screws and collars running in grooves, when its distance from the wires is adjusted. In this way, one has the whole aperture of the tube; and no greater length than is absolutely necessary for use; which, in such an instrument, appeared to me to be an advantage. In some respects I find it so: yet, the hazard of disturbing the collimation, by touching the outside of the tube, is an objection.

The wires are not in one cell; but in two distinct cells, with their faces towards each other. The perpendicular wires are 5, at 35 seconds of time distance in the equator; and are adjustable horizontally for collimation by a screw. The horizontal wires are 3, at about 15 minutes of a degree asunder; placed so as just not to touch, but to pass clear of the other wires; and they are adjustable in collimation by another screw peculiar to them. The two cells have each a power of turning separately on the axis of vision; but, when once the two sets of wires are brought to be truly at right angles to each other, the cells can then be fixed together, and turned together, and finally settled in their place by screws and collars at the outside of the tube. These things, I believe, are new: I thought they might be improvements on the usual method; yet I find the adjustment of the horizontal wires in collimation, might be dispensed with.

My reason for having 3 horizontal wires, and at about that distance, was, that after having ascertained what the difference is, I might observe the lower limb of the sun or moon at the one, and the upper limb at the other of the extreme wires, without much altering the elevation of the telescope, and removing the centre of the object, or preceding and subsequent limbs of the sun or moon, far out of the centre of the field.

The divisions on the circle itself come now to be spoken to. They were done by hand; and have been executed with great care. The original divisions are by dots or points, at every ten minutes. Within, is another row, by strokes or cuts; laid off from the points to every ten minutes likewise. The dots are what we will regard first: the cuts afterwards.

As it always appears to me convenient, in actual observation, to contrive that every thing shall do itself, as far as I can, and to leave the mind as well as the body at perfect ease, and totally disengaged from calculation; I considered, that making both the microscopes talk the same language, read off the same way, with the guiding figure always to the same hand, and the dot to be observed to the same hand too, and the readings always positive, would conduce much to one's ease, and thereby very greatly indeed to the accuracy and certainty of the observation.

With this intent, since the microscopes are, the one above, I ordered that to be marked A; the other below, B; considering that the numbers deduced from them could never be mistaken, if one got into the habit of examining A first, and noting that down, and then examining and setting B under it; which, if all things are true, ought to be the complement to 90 degrees.

To make the reading pleasant, I ordered the micrometer-screw in each to be placed on the right hand; and considered the moveable wire as always to be kept to the right hand of the other. This will of course, in all cases, measure the distance of the fixed wire from the nearest dot apparently on the right, (or, since the microscopes invert, the nearest dot really to the left) which will be either the degree itself on that hand, or some multiple of ten minutes from it.

That the numbering of the degrees might coincide with this idea, I considered, that the figures should be made to appear erect in the microscopes, in every position of the telescope (which they might be whenever it does not point below the horizon) and that they should be reckoned backwards. To effect this, they ought to be reckoned backwards in themselves, but to stand the contrary way, or inverted in reality. This would be different in the two microscopes, in respect of the centre of the circle; but that could create no difficulty. For, since the two quadrants nearest to the object-end of the telescope, would always be those coming under the examination of microscope A; and the two nearest to the eye-end, those to be observed at microscope B; they might be figured accordingly. Hence, supposing the instrument placed in the meridian, with the graduated face turned towards the east; if, when the telescope is horizontal and points to the south, the upper quadrant nearest to the object-end, be numbered from that end from 1 to 90°, with the heads of the figures towards the centre of the instrument; and the other upper quadrant be numbered from the eye-end, with the feet of the figures towards the centre; they both would give the zenith distances of the objects observed. The former, at microscope A, while the telescope points to the south of the zenith; the latter at microscope B, when you are observing towards the north.

The two other, or lower quadrants, follow a similar rule, and serve to shew the altitudes, if both be numbered from the quadrature, instead of either end of the telescope; those leading towards the object-end, being placed with their heads, while those towards the eye-end, stand with their feet towards the centre of the circle.

To render this more intelligible, I will annex a drawing of the divisions, (Tab. XVI. fig. 2) numbered indeed only to every ten degrees; though the instrument itself has a figure at every degree, that one may always be in the field of view of the microscope. Hereby it may be seen, that all on one side of the telescope give zenith distances, while all on the other side give altitudes; and yet, that the figures in both the quadrants nearest to the object-end are placed with their heads towards the centre, and all towards the eye-end with their feet. This became necessary; and though it was a little perplexing at first to contrive, and see executed properly, it is found very convenient indeed in use.

The interior divisions, or cuts, are also numbered at every degree each way, from the eye-end to the object-end of the telescope, with the feet of the figures always towards the centre. The use of them is likewise very great; not for reading off the observations, but for setting the instrument. For, at a proper distance from the main pillars, there is a small pillar, carrying a compound microscope with a wire in its focus; which being adjustable, and once set to the latitude of the place, gives immediately the north polar distance of any object seen; or, by fixing the instrument according to the polar distance of an object sought, one is certain of its entering, at the proper time, the field of the telescope, near the centre wire. This pillar for the polar microscope, is removeable to the other side of the main pillars; which becomes necessary when the instrument is reversed.

This in general is the form, and these are the peculiarities in the construction of this instrument; which, being designed for meridian observations, or transits, I apprehend may best be named a TRANSIT CIRCLE.

In the progress of it, when the divisions came to be examined in their proper position, as to the truth of the opposite dots being exactly in the diameter of the circle, an error was discovered, which occasioned a great deal of trouble, and much loss of time. When the microscopes had been adjusted with care, after turning the circle one way, they continued true, and the same dots shewed themselves to be perfectly in the diameter, however often the circle were turned the same way round: but on one or more revolutions the contrary way, the same dots ceased to appear true. This, it was thought, could arise only from some deviation in the centre. And, since the figured accordingly. Hence, supposing the instrument placed in the meridian, with the graduated face turned towards the east; if, when the telescope is horizontal and points to the south, the upper quadrant nearest to the object-end, be numbered from that end from 1 to 90°, with the heads of the figures towards the centre of the instrument; and the other upper quadrant be numbered from the eye-end, with the feet of the figures towards the centre; they both would give the zenith distances of the objects observed. The former, at microscope A, while the telescope points to the south of the zenith; the latter at microscope B, when you are observing towards the north.

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Y*s* hanging in gimmals was a new experiment, this error was supposed to take its rise from some *shake* in them. They were examined; and were altered in various ways. Fixed Y*s* were then made, of the usual form; others of a larger; others of a more acute angle. The difficulty was still thought to continue. Recourse was then had to Y*s* in gimmals again, which I was unwilling to give up; and friction-rollers were applied to take off some of the weight. Still this error did continue in a small degree: yet was that degree so small, as not to be discernible at the polar microscope; nor, as far as I could see, at those belonging to the plumb-line; and sometimes scarcely so at the others, to whose greater magnifying power it seemed to be owing that it was at all perceptible. The cause I then supposed to be, in a disposition in the pivots to gather up the side of the Y*s* towards which they were turned. Yet was that not the cause: for what little motion there was, I found afterwards to be in a contrary direction.

This led me into discovering, and at last rectifying the defect. The original idea of hanging the Y*s* in gimmals, as was said before, was derived from Mr. SMEATON; who kindly shewed to Mr. CARY those which he had made to a small transit instrument for his own use. His ought scarcely, in strictness, to be called Y*s*; for he had made a little hollow on each side where the pivots would touch, as a sort of bed to receive them, and make the angle less pinching. This, Mr. CARY had imitated: and, though I did not mean he should, he did the same to the second pair he made, after trying the other kinds. Since it was done, I let them so remain till I got the instrument home; for I really found all trials so disturbed by the shaking of carriages, while it was at his house, that I could make no satisfactory examination there myself. When the instrument was in its place, I tried every experiment I could contrive to discover the cause of this error; whether it could be in the microscopes themselves; any shake in them, or in the pillars, or in the hanging of the Y*s*. Finding none of these to be in fault; and, upon trying the instrument at every 10 degrees all round, perceiving the axis thrown backward instead of forward upon turning either way, it occurred to me, that any grease or other particles would have it more in their power to produce that effect in a sort of pivot-hole, (which the hollowed sides really are) than between two fair flat surfaces. I thereupon took out the Y*s*, and had them formed to an exact right-angle, with the whole sides perfectly smooth, and flat, and well finished: and, since that has been done, I really can discover no difference which ever way the circle be turned; but think I may now say that deviation is quite removed.

Yet, I apprehend, it would have been of no consequence if it had continued, or been greater than it was. For, since the readings are as it were in a line above and below the centre, and both of them positive; any motion of the centre towards the right hand, would give the dots, both above and below, the appearance of being more to the left than they ought to be; and thence would give the measurement too small, and that in an equal degree in each; so that the sum of zenith distance given by one microscope, and of altitude by the other, would thereby be less than 90 degrees, by just double the error. And if the axis be moved towards the left, the contrary would be the result; the sum would exceed 90 de-

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Yet, I apprehend, it would have been of no consequence if it had continued, or been greater than it was. For, since the readings are as it were in a line above and below the centre, and both of them positive; any motion of the centre towards the right hand, would give the dots, both above and below, the appearance of being more to the left than they ought to be; and thence would give the measurement too small, and that in an equal degree in each; so that the sum of zenith distance given by one microscope, and of altitude by the other, would thereby be less than 90 degrees, by just double the error. And if the axis be moved towards the left, the contrary would be the result; the sum would exceed 90 degrees, by just double that quantity. Hence, the difference from 90 degrees, at the same time that it gives a mean between the two readings, would reduce the error or deviation of the axis to nothing.

The instrument here described, is of the size I thought would be most convenient for my own use: indeed it is full as large as I should recommend ever to be made in that moveable form. It stands on a cylinder of one solid stone, of $25\frac{1}{2}$ inches diameter, and 3 feet 6 inches long, bedded on a pier of brick, well bonded together, and rising from a good foundation, deep in the earth. The stone is clear of the floor all round, and is very steady indeed: the instrument rarely varies at all, in any respect. It is adjusted in the meridian to two marks, the one north, the other south; so that now they are truly placed, the collimation of the telescope is easily examined, without lifting the circle out of its Y*s*.

I may be supposed partial to an idea which I have long entertained; but I confess, I should very strongly recommend the having an instrument of this nature, though more perfect, in every observatory; I mean a transit instrument, on stone piers, with a suitable circle and microscopes; that, whenever you observe a meridian passage, you may, at the same time, measure the exact altitude, or zenith distance of every object seen. The being obliged, in the common way, to have recourse to two different instruments, occasions the zenith distances to be much less frequently observed, than it is to be wished they were. It is true the British catalogue was, for the most part, deduced from observations with a quadrant alone; and so was MAYER's. But, though labour and patient perseverance, may enable an observer to allow for any deviations in the limb, a quadrant is at the best but an imperfect instrument for right ascensions.

I believe, in the best observatory, I should confine myself to a telescope of 45 inches, with a circle of about 3 feet 6 inches. Such a telescope would have great power; and the whole would not be encumbered with too great weight. If the telescope be of 5 feet, the circle must be of 4 feet 6 inches. But I certainly should dissuade the ever going beyond that; and I doubt whether the great additional weight of metal, and the disparities there will be in such a mass, would not counteract the advantage of a longer telescope. Beside, it may deserve consideration, that in a larger instrument some parts may be out of the extent of the observer's arms, which he could wish to reach. Our late friend, Mr. SMEATON, was against a circle of above 3 feet diameter. Between stone piers, there must be a double apparatus of microscopes, &c. to use when the instrument is reversed.

In conclusion, it may perhaps be proper to add, since some gentlemen may feel inclined to ask, How my instrument has performed? whether in actual observation it does what was expected from it? To this, I think, I now may fairly answer in the affirmative; that I do find it a very useful instrument, and the best adapted, of any that I know, to the perfecting of our catalogues. For some time, I will confess, I had my doubts. I received it in the beginning of the winter, when the cold and dark weather made all examination of it irksome. As a transit-instrument, I soon was satisfied with its performance, even in respect of the pole star itself. It is very steady; and rarely wants any re-adjustment at all. As a circle, I was not. The deviation of the axis, though both ends, as far as I could judge, seemed always to deviate equally, perplexed me much; and destroyed all confidence. The collimation in altitude (whose error when constant, is unimportant) appeared variable; and seemed to give uncertain conclusions. Whether that took its rise from the object-glass, or the wires; from the hanging of the plumb-line, or the microscopes; was doubtful. All these things it took up much time to investigate. But I think I now may say, these difficulties are all surmounted. The error in the Y*s*, it has been said already, is cured. The object-glass I suspected might have some little *shake*, from its being attached to a false tube on the outside, and therefore liable to be touched, instead of being within the tube of the telescope itself. This was made more certainly steady. The wires I was sure did not move. Neither did the microscopes, after I had set them perfectly at ease; for I found that in the first placing, I had a little cramped one of them. The dots came then to be examined, by which the plumb-line is adjusted; or rather, I should say, by which the circle is brought into a position for adjusting the microscopes. Here a small error was discovered. It has been mentioned already, that there are four pair of dots for this purpose. Though laid off, I am satisfied, at first with the greatest care, and strictly true; the opening or enlarging of them afterwards, to make them just visible on each side of the plumb-line, had occasioned some very small differences in them, in respect of their adjoining divisions or dots on the limb. The adjusting therefore to a different pair of these dots, which I had done, would necessarily occasion a difference in the collimation. This being avoided, by using always the same; and other causes of error being removed; the collimation for altitude seems now to be as steady as can well be desired. I had more of these dots; because, the instrument being new, I could not be certain in what position it might be found most convenient to adjust. I now do it always with the telescope pointing to the zenith; and in another instrument, I should recommend the having no more than two dots for that purpose.

Some small errors I do perceive, which I believe are to be ascribed to the great power of my microscopes, which are too strong for works of art. It was against the judgment of Mr. Cary that they magnified so much; and I believe he was in the right. Some errors are certainly to be laid to the charge of my own eyes, which do not define objects as they used to do. But in general, I may fairly say, my observations of the same star, seldom differ from each other above 5 seconds in altitude, and most commonly they are much within that limit.

In observing, I always study to be as much at my ease as possible: and therefore I always sit, and use a prismatic eye-glass. To avoid touching the instrument itself, or even the stone on which it stands, I have four upright poles from the floor to the roof, with cross braces on a level with the bottom plate of the instrument; against which I may lean, while I observe, or when I handle any part of the instrument. These I find to be of great comfort and use. Against two of the poles I hang a curtain occasionally, to keep off the sun, or to lessen the false light when I observe a star in the day.

The two exterior horizontal wires, mentioned above, I find very convenient. They are really 14′ 43″,5 of a great circle distant from the centre. By means of them, I can, without any hurry, observe the preceding limb of the sun at 3 wires;

then set the lower limb to the upper wire, and read that off; set the upper limb to the lower wire; am ready to observe the second limb of the sun at the 3d, 4th, and 5th wires; and lastly, read off the upper limb after the observation is ended. In this way, one has the meridian passage through the middle of the field, or within 2′ of it: and the meridian altitude of both the limbs, while the sun's centre is on the meridian; for the little alteration in altitude is soon done, and can disturb nothing.

Indeed, upon the whole, this instrument itself is capable of doing a great deal of good work; and convinces me fully, that one between piers would be highly advantageous to astronomy. As a transit, mine is perfect, so far as that size permits: indeed it is in fact to all intents a transit-instrument. And for altitudes; since the readings are totally independent of the circle, though you have it in your power to re-examine your microscopes by the plumb-line between each observation, if you please; you find there is no occasion for it. In that respect, it has the advantage over a quadrant. No force is used in setting this instrument: the whole, from its form, is counterpoised in itself: there is no more probability of deranging it in altitude, than in azimuth: and therefore, all you have to do in actual observation beyond a common transit-instrument, is, to bisect the star as it passes, or as soon as ever it has passed the meridian wire, and read off the microscopes afterwards. Thus every observation is complete; by ascertaining the right ascension and altitude of every object at once, and with very little trouble; which must tend greatly to the improvement of our catalogues.

There is one additional advantage in an instrument of

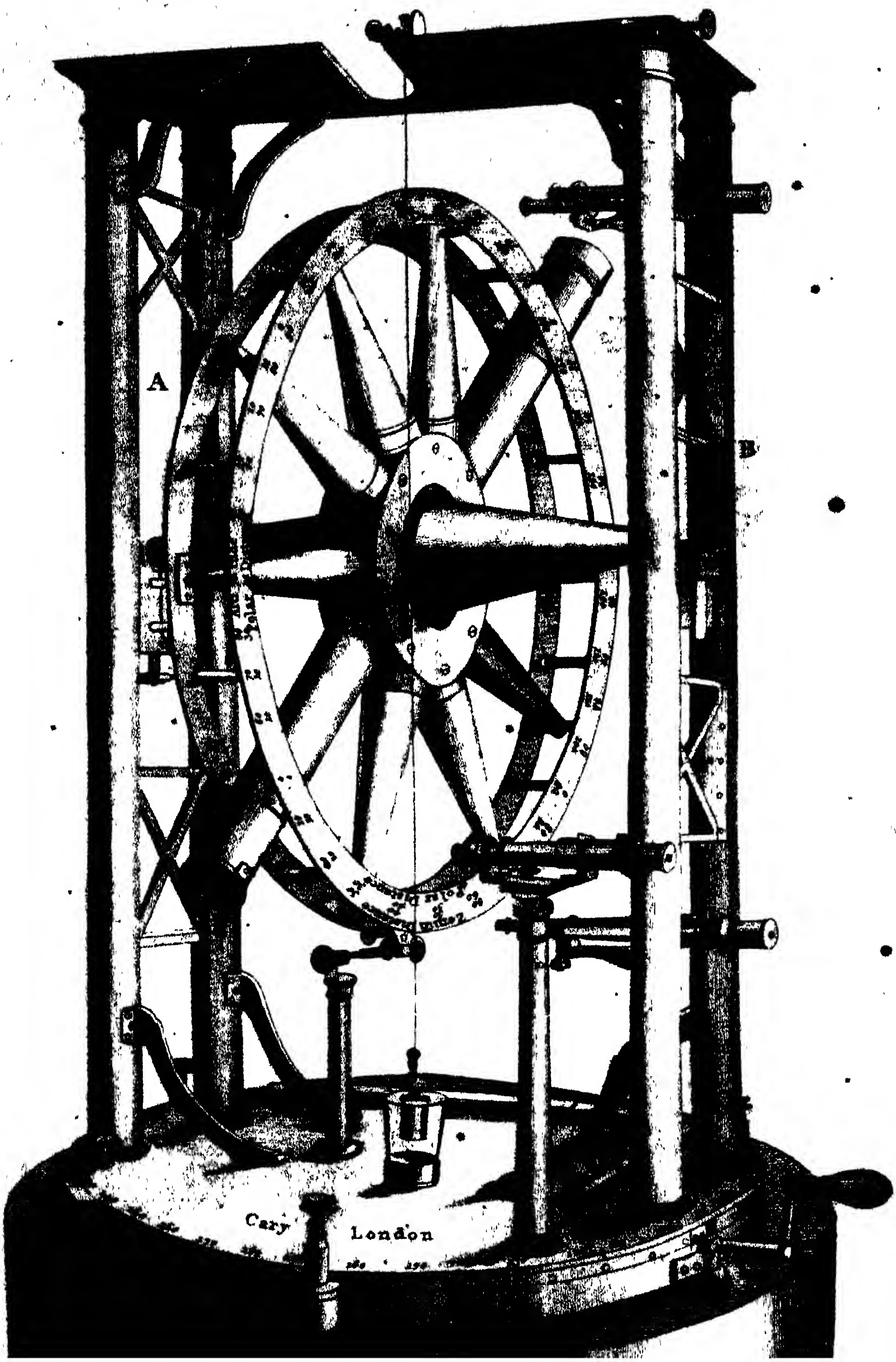

A
B
Cary
London

Fig. 1.

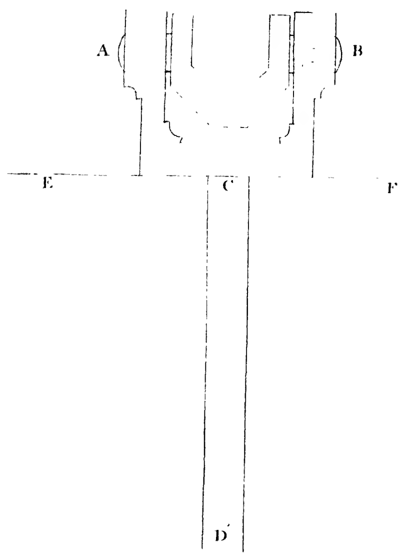
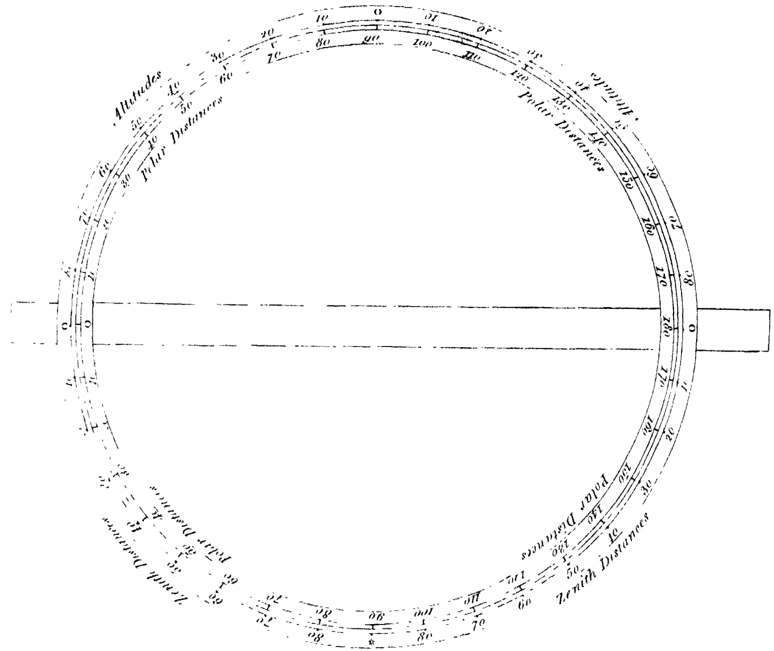


Fig. 2.



this form; that you have it in your power to reverse the whole in a few minutes without any hazard; which I do regularly; because thereby you discover, and destroy, any errors which there may be in the instrument itself, or which may at any time arise in observing.

XIII. *Description of an extraordinary Production of Human Generation, with Observations. By* John Clarke, *M. D. Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read May 16, 1793.

In the course of the last year, a woman was admitted into the General Lying-in Hospital, in Store Street, Tottenham-court Road, who, after a natural labor, was delivered of a healthy child.

The birth of this child was succeeded, however, by a repetition of uterine contractions, by which another substance was expelled, which is the subject of this paper.

It was inclosed in a distinct bag of membranes, composed of decidua, chorion, and amnios, and had a placenta belonging to it; the side of which was attached to the placenta of the perfect child. The membranes had been opened before I saw it, and a small quantity of liquor amnii having been discharged, the contents of the cavity were exposed.

The substance contained in the membranes was of an oval figure, rather flattened on the two sides. Its long diameter was about four inches; and its short diameter, from edge to edge, three inches. One edge was rather more concave than the other, and near the centre of it there was a small and thin funis, in length about an inch and half, by means of which it was connected to the placenta.

The surface of this substance was covered with the common integuments, and from it issued four projecting parts. Of these the upper is an imperfect resemblance of the foot of a child, having one large and three smaller toes upon it. The lower is a still more imperfect imitation of a foot, having one large and two smaller toes.

Between the two feet is situated a small and rounded projection; into which a small passage led, capable of containing a large bristle, but it soon terminated in a *cul de sac.* Close to the funis there was another small and thin projection, about a third of an inch in length, which looks like a finger, and was found to contain bony matter, and joints.

There was no appearance of head, or neck. No ribs could be felt, nor clavicle, nor scapula. There was no vestige of any thing like legs, or thighs, or upper extremities; or of organs of generation.

The only external similarity of this monster to a human fœtus, consists of its covering, and the attempt at a formation of two feet, and a finger.

Before the internal structure was examined, the navel-string of the perfect fœtus was injected, from whence the injection very readily passed through both placentæ, viz. that of itself, and that of the monster; and then into the substance of the monster also, as appeared by the redness of the skin.

When the injection had become cold, the skin was carefully dissected off; in doing which it was found that the upper foot had no bony connection, but grew loose, and only connected to the internal parts by cellular substance. The lower foot was articulated to the inferior part of the tibia and fibula.

The internal structure of the monster was composed of soft and bony matter. Upon cutting into the former, it appeared of a homogeneous fleshy texture, but without any regular or distinct arrangement of muscular fibres; and was very vascular throughout.

The bones which were surrounded by this fleshy substance were, the os innominatum, the os femoris, the tibia, and the fibula. The relative situation of these to each other described the attitude of kneeling. With regard to the bones themselves, the os innominatum, and the os femoris are both perfect, and of the size which we meet with in a fœtus at the full period of utero-gestation; but the tibia and fibula are much shorter than in their natural proportion to the thigh bone.

At the upper part, and towards the inside of the os innominatum, was placed a little portion of small intestines, loosely connected, by their mesentery, to the posterior edge of that bone, where it is commonly united to the os sacrum. These intestines had a covering of peritonæum, and were very minutely injected.

The next object was to trace the vessels of the funis, which was done with great care. There appeared to be only two, viz. an artery, and a vein; and these passed on towards the inner surface of the os innominatum. As they approached this bone, they gave off some branches to the surrounding parts, which quickly became too small to be traced. The trunks then passed backward, towards that part where the articulation with the os sacrum is generally found; at which place they went to the other side of the bone, where they distributed a great number of small branches, and were at length lost in the surrounding parts.

This was the whole of the internal construction of this very extraordinary monster. There was not the smallest appearance of head, or vertebræ, or ribs. There was neither brain, spinal marrow, nor nerves. It had no heart, nor lungs. It contained none of the viscera subservient to digestion, excepting the intestines already mentioned; nor any glandular substance whatsoever.

This being a monster of so singular a nature, I shall beg leave to add, to the foregoing description, a few observations, which the circumstances appear to me naturally to suggest.

The mere description of any monster is of very small utility, unless it tends to explain some actions of the animal economy, before imperfectly, or not at all understood. It is on this account that very little addition has been made to the stock of our knowledge of natural history, from considering those monsters in which there are either supernumerary or confused parts; because, if we cannot distinctly perceive the use, or necessity of parts, in their natural state, we are not likely to advance in information by the examination of those varieties of structure, where difficulties are only multiplied by the greater complication, or aggravated by the confusion of parts. The only useful inference in natural history, which can be drawn from monsters of the last kind is, that nature can deviate from the usual arrangement of parts, without any material inconvenience; and therefore, that the existence of parts so as to be capable of being applied to the purpose for which they are intended, in the perfect state of the system, rather than any precise order of them, is required for carrying on the functions of an animal body.

Monsters, however, where considerable parts are wanting, seem peculiarly likely to assist in the prosecution of physiological researches.

If we were never to see an animal except in its perfect state, we could form no just idea of the comparative necessity of the different parts. So also, if we were to attend alone to the complete structure which obtains in the more perfect animals, we might be led falsely to conclude, that the usual connexion of parts, which we find in them, was essential to the structure and composition of animal matter. Of these parts, the brain and nerves, the stomach and digestive organs generally, the heart, and the lungs, would appear to be of such importance in the machine, that one would be induced to imagine that the functions of life could not be carried on without them: but in tracing the works of nature downwards, we shall at length find animals gradually becoming more and more simple in their construction. The brain and nervous system are altogether wanting in some, and there are others which have neither heart nor lungs; yet they continue to exist, and are capable of performing the most important functions of animals. Thus the formation of one animal serves to throw light upon the economy of others.

This great simplicity of structure is found, however, chiefly in animals the texture of whose bodies is nearly homogeneous; not consisting, as in more perfect animals, of parts so different from each other, as skin, intestines, &c. are from bone.

It might therefore still be supposed, that all the complicated mechanism, found in the more perfect animals, is essential to the construction of such heterogeneous substances, as those of which they consist.

To investigate this matter, we must have recourse to those monsters in which there is a deficiency of parts.

There is a very material difference between the nature of the life of the more perfect animals, during their time of fœtal existence, and after that they are born. In the latter state, the brain and nerves appear to be so essential, that any very considerable defect in them is incompatible with the well-being of the animal; but in the uterine state, considerable deviations from the ordinary arrangement of parts, and such as cannot be endured after birth, are supported without any inconvenience.

The brain has been frequently found very incompletely formed, and sometimes not at all, yet still there have been nerves. In other cases, where the brain has been perfect, the spinal marrow has been deficient in a great part of its extent, and sometimes throughout.

Both these occurrences are sufficient to prove, that, at any rate, that intimate connexion of the brain and nervous system, which takes place after birth, is not necessary for the formation of a body in other respects perfect. But still it would remain doubtful, whether any regular structure could be formed, without any vestige of either brain, or nerves; and therefore without a possibility of their influence, in any manner, toward such structure.

The monster now under consideration is so extremely simple, in this respect, that it cannot be exceeded by the most simple animal known.

It may be objected, however, that there might be brain, or nervous fibres, in this monster, but that they might, in the dissection, be destroyed. But, in the first place, I beg leave to observe, that the parts were examined too carefully for such a suspicion; and, in the next, as there were no bones representing either the cranium, or spine, or os sacrum, it is not probable that their contents should exist in any other situation.

Another objection may perhaps be taken from the anastomosis of the vessels of the monster, with those of the perfect fœtus, and it may be assumed, that the nervous influence might be transmitted, in this way, along the vessels; but there is very good reason for believing that the vessels of the placenta have no nerves, since, when we cut the navel-string, neither the mother, nor the child, expresses the smallest sign of sensation: and indeed, even if they had nerves, it is still very unlikely that, merely by such anastomosis, any nervous influence could be conveyed.

I think it right to answer another possible objection which may be made, viz. that nervous matter may be co-extended, or co-existent with all other animal matter, and that, of course, it is of no consequence whether there be any sensorium, or reservoir of impressions, &c. or not; because the stimulus, which produces action, must reside in parts, as well as the other substance of which they are composed.

Now, although this may possibly be true, we have no evidence of the fact sufficiently satisfactory to carry conviction along with it. On the contrary, there seems to be good reason for entertaining an opinion, that nervous influence is conveyed from the brain downwards. If we are right in this conjecture, which is warranted by the experiment of tying, or cutting nerves, then the existence of the nervous fibre, like that of a string of a musical instrument, would be inactive, unless it received an impression, which, with regard to the nerves, should come from the brain.

The whole of the actions of this monster, then, must have been those of the vascular system entirely; and these seem to have been capable of forming bone, skin, cellular substance, ligament, cartilage, intestines, &c.

The defect of heart (not an uncommon kind of monstrosity) proves, that the energy of the arteries was equal to carrying on the circulation, not only in its own body, but also through its own placenta.

The deficiency of nerves renders it extremely probable that their use is very small, if any, to the embryo.

It has been an opinion, entertained by a very acute physiologist, Mr. JOHN HUNTER, that, in all cases, a fœtus is a very simple animal, as to its internal actions, and the circumstances attending this monster fully confirm his idea.

The usual objects of nature in the formation of a fœtus are, that it should grow, and that it should be fitted with parts which, though of no use to it then, are essential afterwards. We know that the lungs are of this kind, and it is very likely that the brain and nerves are so too.*

The common uses of the nervous powers are, to convey impressions from without, and volition from within. Now a fœtus in the uterus is exposed to no external impressions, and is most probably incapable of volition, since it is not con-

* That there is a very material difference between the internal functions of a fœtus in the womb, and those of an infant after birth, seems very presumable; not only from finding that it can carry on life without parts which are of the greatest moment afterwards; but also from its possessing parts which after birth go into decay, or disappear, as the thymus gland, &c.

formable to the general wisdom of nature to give that which, in such a situation, must be useless.

The whole growth then, and formation of a foetal body, would seem to depend upon the actions of the vascular apparatus, which, if we may be permitted to judge from this instance, is fully equal to the task.

With regard to the manner in which this monster was supplied with nourishment, and with the benefit of air, there is nothing remarkable; because it had a placenta, and the circulation between it and the mother was the same as in the most perfect foetus.

Tab. XVII. and XVIII.

Exhibit the appearances described in the foregoing paper.

Tab. XVII. A view of the external appearances.

A. An imperfect formation of a foot, with four toes upon it.

B. An imperfect formation of another foot, having three toes upon it. This foot was connected to the tibia and fibula.

C. The projection into which a duct led, terminating in a blind pouch.

D. The funis umbilicalis.

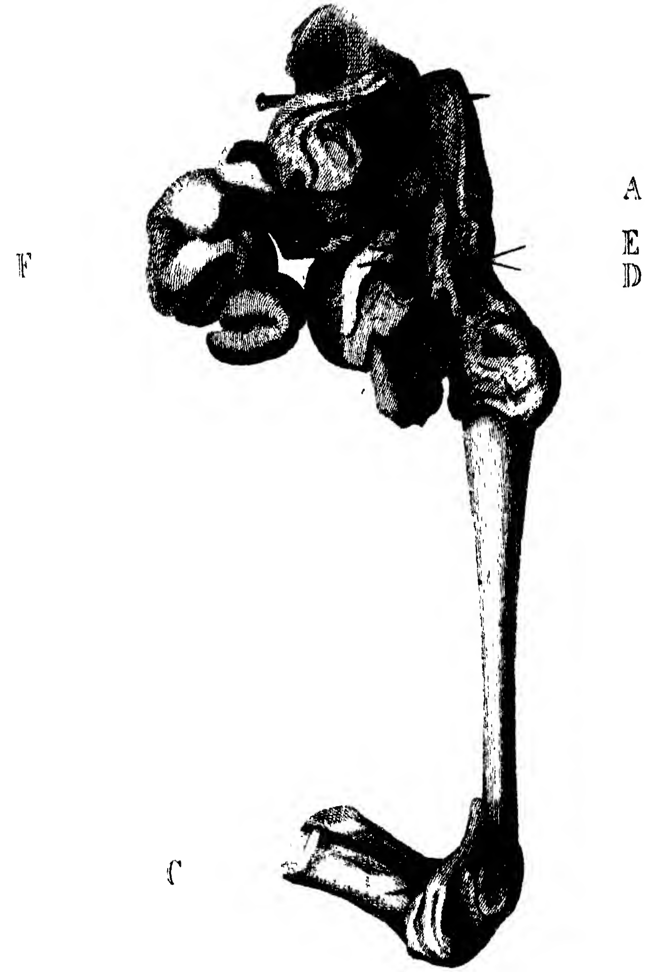
E. An imperfect formation of a finger.

Tab. XVIII. An internal view of the parts, as they appeared after clearing away the fleshy matter from the bones.

A. The os innominatum.

B. The os femoris.


A
E
D
C
B


A
E
D
F
C

C. The tibia and fibula, to which the lower foot was connected.

D. The funis umbilicalis, with two bristles in the vessels.

E. The bristles passing, in the vessels, to the outside of the os innominatum.

F. A portion of small intestines, terminating in a *cul de sac* at each extremity.

XIV. *Description of an Instrument for ascertaining the specific Gravities of Fluids. By* John Godfrey Schmeisser. *Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read May 16, 1793.

ALTHOUGH it be well known to chemists, as well as to experimental philosophers in general, that the ascertaining the specific gravities of bodies is a matter of great importance in various chemical experiments, as well as in the analysis and chemical investigation of different substances; yet we find that this precaution is too frequently neglected in the accounts given of the experiments by the authors themselves, and that the neglect of it has sometimes occasioned the failure of these very experiments, when repeated by others.

As this defect has, in a great measure, arisen from the want of an accurate and convenient apparatus, a defect which I have formerly experienced myself, I have for some time past employed my thoughts in inventing a contrivance by which this difficulty might be removed. I flatter myself that I have now succeeded; having contrived an instrument, which I have found to answer every purpose for which it was intended, to my great satisfaction; so that the specific gravities of fluids may be determined in an easy and accurate manner.

Every ingenious man will easily convince himself in what respect this instrument may differ from, or how far it may be preferable to, those which have been hitherto made public; and even to that lately invented and recommended by Mr. RAMSDEN.

The whole apparatus is represented in Tab. XIX. fig. 1; it consists of a flat-bottomed glass bottle (fig. 2) in which is fitted, by grinding, a glass stopper having a thermometer passing through it, (fig. 3.) The bore of this stopper is conical, (fig. 4) and the thermometer has a glass collar, (fig. 5) which is ground into the bore of the stopper, so as to be perfectly tight. There is some difficulty both in making the glass collar, and in fitting it into the stopper. If the thermometer tube and the collar be not made of the same metal, the collar is very apt to fly off in grinding; for this reason I have sometimes fixed the tube into the stopper by means of a thin piece of elastic gum, wound very tight round the tube. This gum, by its elasticity, effectually excludes air and liquids, and is, in the usual temperature of the atmosphere, not dissolved by any liquor, except vitriolic æther, and not even by that, unless it is particularly prepared for the purpose.

The cavity left at the upper part of the stopper may be filled up with sealing wax, or any other kind of cement; this will assist in fixing the tube, and as the liquors to be weighed do not come in contact with this part, if the bottle be carefully filled, there is no danger that the wax, or cement made use of, should in any degree affect the accuracy of the experiments.

I have made, at different times, comparative experiments with this instrument, with a view to the further ascertaining its accuracy, and the different improvements made in it; and I can with much confidence assert, that I have never found either the least difference in the results, or any thing else contrary to my expectations.

The manner of using this instrument, and preparing it for experiments, is as follows.

(1.) A. An accurate cubic inch, which is fastened, by means of a horse-hair, to a hydrostatic balance, is to be suspended in a vessel with distilled water, of the temperature of 60 degrees, according to FAHRENHEIT; when the sum of the weight which the cubic inch thus loses, in the water, will be equal to the weight of an equal quantity of water displaced by it.

(2.) B. The instrument, free from moisture, is then to be put into the scale of an accurate balance, and its weight ascertained, from which the weight of the common air contained in the bottle must be deducted; when the remainder will indicate the absolute weight of the instrument.

(3.) C. The bottle of the apparatus is then to be filled with distilled water, of the temperature of 60 degrees, and the stopper, with the thermometer, fitted to the bottle, so that neither the smallest bubble of air may remain in it, nor any of the fluid adhere to the outside of the stopper or bottle; after which the weight of the water is to be ascertained, and marked upon the bottle, from which, by calculation according to experiment A, the quantity of water, contained in the bottle in cubic inches measure, may be found. Having thus ascertained the quantity of water of 60 degrees of temperature which the bottle contains, the bottle may then be filled with any other fluid of the same temperature, and its weight ascertained, according to experiment C, and compared with that of distilled water. If, for example, the bottle be found

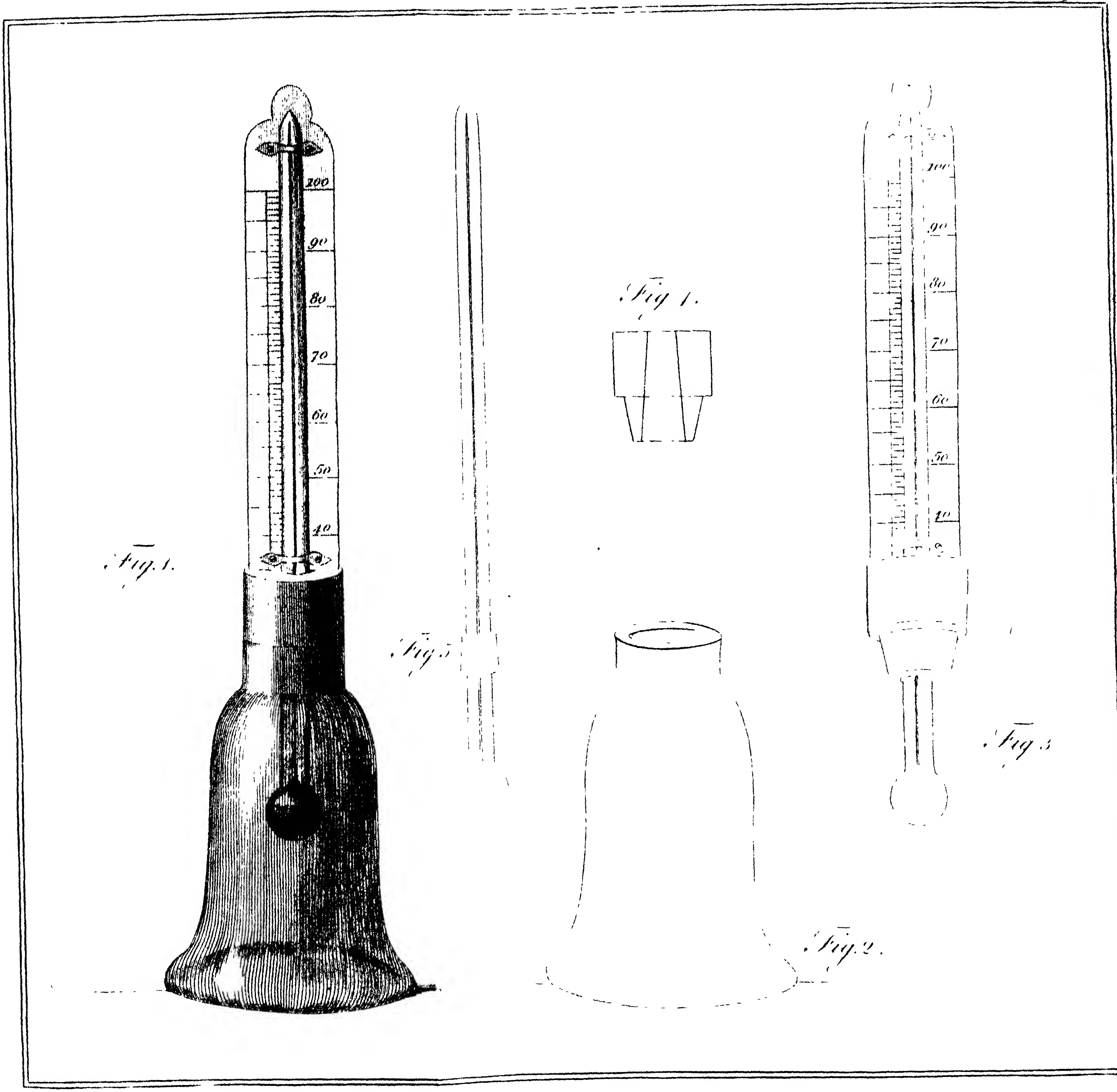


Fig. 1.
Fig. 5
Fig. 4.
Fig. 2.
Fig. 3

to contain 327 grains of distilled water, and 654 of another fluid, the difference will be as 1 to 2; or 654 divided by 327, will give 2 for the quotient. The specific gravity then, of the fluid thus found, compared with that of distilled water, is properly expressed by the ratio 2,000 : 1,000; which latter expression is taken for the standard.

As it is a known fact that fluids exhibit different specific gravities at different temperatures, it would have been necessary for me to form a table, exhibiting the specific gravities of fluids at different temperatures, had I not, in order to avoid this inconvenience, hit upon a method of bringing the fluids, whose specific gravities are to be investigated, to a certain standard, viz. to 60 degrees, by setting the bottle with the fluid in a glass vessel with cold water, and adding as much warm water as may be necessary to bring that fluid to this standard of 60 degrees.

As the fluor acid will in some measure dissolve the glass, it becomes necessary, when that acid is to be weighed, to coat the inside of the bottle, by melting a little bees-wax in the bottle, and turning it, with the thermometer, in such a manner that the inside, together with the lower part of the thermometer, may become totally covered when cooled; which coating may easily be removed by means of a little oil of turpentine, or any other essential oil, all of which dissolve wax very readily.

XV. *Extract of a Letter from Sir* Charles Blagden, *Knt. Sec. R. S. to Sir* Joseph Banks, *Bart. P. R. S. giving some Account of the Tides at* Naples.

Read May 2, 1793.

Rome, March 30, 1793.

I TOOK some pains at Naples to get information about the state of the tides, but could learn nothing satisfactory. The quantity of rise and fall is so little, that unless the sea be very calm, it is impossible to make a good observation. One of the best places for ascertaining the phænomena would be at what they call the river Styx, which is a narrow communication between the Porto di Miseno and the Mare Morto. Here I learned very distinctly that the water sometimes ran in, and sometimes out, but could not get the times; when I was there it was running out. The best observation I had was on the 2d of March, when it appeared to be high water at Naples about eleven in the forenoon, and low water between five and six in the afternoon; with a difference of pretty exactly one foot in the height. The wind blew the same way all the time, and the sea was very little agitated. On the preceding day the water had sunk an inch or two lower. From this observation, as well as some others less accurate, I concluded the time of high water at full and change to be between nine and ten o'clock, in the Bay of Naples.

I am, &c.

C. BLAGDEN.

XVI. *Observations on Vision.* *By* Thomas Young. *Communicated by* Richard Brocklesby, *M. D. F. R. S.*

Read May 30, 1793.

It is well known that the eye, when not acted upon by any exertion of the mind, conveys a distinct impression of those objects only which are situated at a certain distance from itself; that this distance is different in different persons, and that the eye can, by the volition of the mind, be accommodated to view other objects at a much less distance: but how this accommodation is effected, has long been a matter of dispute, and has not yet been satisfactorily explained. It is equally true, though not commonly observed, that no exertion of the mind can accommodate the eye to view objects at a distance greater than that of indolent vision, as may easily be experienced by any person to whom this distance of indolent vision is less than infinite.

The principal parts of the eye, and of its appertenances, have been described by various authors. Winslow is generally very accurate; but Albinus, in Musschenbroek's *Introductio*, has represented several particulars more correctly. I shall suppose their account complete, except where I mention or delineate the contrary.

The first theory that I find of the accommodation of the eye is KEPLER's. He supposes the ciliary processes to contract the diameter of the eye, and lengthen its axis, by a muscular power. But the ciliary processes neither appear to contain any muscular fibres, nor have they any attachment by which they can be capable of performing this action.

DESCARTES imagined the same contraction and elongation to be effected by a muscularity of the crystalline, of which he supposed the ciliary processes to be the tendons. He did not attempt to demonstrate this muscularity, nor did he enough consider the connection with the ciliary processes. He says, that the lens in the mean time becomes more convex, but attributes very little to this circumstance.

DE LA HIRE maintains that the eye undergoes no change, except the contraction and dilatation of the pupil. He does not attempt to confirm this opinion by mathematical demonstration; he solely rests it on an experiment which has been shewn by Dr. SMITH to be fallacious. HALLER too has adopted this opinion, however inconsistent it seems with the known principles of optics, and with the slightest regard to hourly experience.

Dr. PEMBERTON supposes the crystalline to contain muscular fibres, by which one of its surfaces is flattened while the other is made more convex. But, besides that he has demonstrated no such fibres, Dr. JURIN has proved that a change like this is inadequate to the effect.

Dr. PORTERFIELD conceives that the ciliary processes draw forward the crystalline, and make the cornea more convex. The ciliary processes are, from their structure, attachment, and direction, utterly incapable of this action; and, by Dr.

JURIN'S calculations, there is not room for a sufficient motion of this kind, without a very visible increase in the length of the eye's axis: such an increase we cannot observe.

Dr. JURIN'S hypothesis is, that the uvea, at its attachment to the cornea, is muscular, and that the contraction of this ring makes the cornea more convex. He says, that the fibres of this muscle may as well escape our observation, as those of the muscle of the interior ring. But if such a muscle existed, it must, to overcome the resistance of the coats, be far stronger than that which is only destined to the uvea itself; and the uvea, at this part, exhibits nothing but radiated fibres, losing themselves, before the circle of adherence to the sclerotica, in a brownish granulated substance, not unlike in appearance to capsular ligament, common to the uvea and ciliary processes, but which may be traced separately from them both. Now at the interior ring of the uvea, the appearance is not absolutely inconsistent with an annular muscle. His theory of accommodation to distant objects is ingenious, but no such accommodation takes place.

MUSSCHENBROEK conjectures that the relaxation of his ciliary zone, which appears to be nothing but the capsule of the vitreous humour where it receives the impression of the ciliary processes, permits the coats of the eye to push forwards the crystalline and cornea. Such a voluntary relaxation is wholly without example in the animal economy, and were it to take place, the coats of the eye would not act as he imagines, nor could they so act unobserved. The contraction of the ciliary zone is equally inadequate and unnecessary.

Some have supposed the pressure of the external muscles, especially the two oblique muscles, to elongate the axis of the eye. But their action would not be sufficiently regular, nor sufficiently strong; for a much greater pressure being made on the eye, than they can be supposed capable of effecting, no sensible difference is produced in the distinctness of vision.

Others say that the muscles shorten the axis: these have still less reason on their side.

Those who maintain that the ciliary processes flatten the crystalline, are ignorant of their structure, and of the effect required: these processes are yet more incapable of drawing back the crystalline, and such an action is equally inconsistent with observation.

Probably other suppositions may have been formed, liable to as strong objections as those opinions which I have enumerated.

From these considerations, and from the observation of Dr. PORTERFIELD, that those who have been couched have no longer the power of accommodating the eye to different distances, I had concluded that the rays of light, emitted by objects at a small distance, could only be brought to foci on the retina by a nearer approach of the crystalline to a spherical form; and I could imagine no other power capable of producing this change than a muscularity of a part, or the whole, of its capsule.

But in closely examining, with the naked eye in a strong light, the crystalline from an ox, turned out of its capsule, I discovered a structure which appears to remove all the difficulties with which this branch of optics has long been obscured. On viewing it with a magnifier, this structure became more evident.

The crystalline lens of the ox is an orbicular, convex, transparent body, composed of a considerable number of similar coats, of which the exterior closely adhere to the interior. Each of these coats consists of six muscles, intermixed with a gelatinous substance, and attached to six membranous tendons. Three of the tendons are anterior, three posterior; their length is about two thirds of the semi-diameter of the coat; their arrangement is that of three equal and equidistant rays, meeting in the axis of the crystalline; one of the anterior is directed towards the outer angle of the eye, and one of the posterior towards the inner angle, so that the posterior are placed opposite to the middle of the interstices of the anterior; and planes passing through each of the six, and through the axis, would mark on either surface six regular equidistant rays. The muscular fibres arise from both sides of each tendon; they diverge till they reach the greatest circumference of the coat, and, having passed it, they again converge, till they are attached respectively to the sides of the nearest tendons of the opposite surface. The anterior or posterior portion of the six viewed together, exhibits the appearance of three penniformi-radiated muscles. The anterior tendons of all the coats are situated in the same planes, and the posterior ones in the continuations of these planes beyond the axis. Such an arrangement of fibres can be accounted for on no other supposition than that of muscularity. This mass is inclosed in a strong membranous capsule, to which it is loosely connected by minute vessels and nerves; and the connection is more observable near its greatest circumference. Between the mass and its capsule is found a considerable quantity of an aqueous fluid, the liquid of the crystalline.

I conceive, therefore, that when the will is exerted to view an object at a small distance, the influence of the mind is conveyed through the lenticular ganglion, formed from branches of the third and fifth pairs of nerves, by the filaments perforating the sclerotica, to the orbiculus ciliaris, which may be considered as an annular plexus of nerves and vessels; and thence by the ciliary processes to the muscle of the crystalline, which, by the contraction of its fibres, becomes more convex, and collects the diverging rays to a focus on the retina. The disposition of fibres in each coat is admirably adapted to produce this change; for, since the least surface that can contain a given bulk is that of a sphere, (Simpson's Fluxions, p. 486) the contraction of any surface must bring its contents nearer to a spherical form. The liquid of the crystalline seems to serve as a synovia in facilitating the motion, and to admit a sufficient change of the muscular part, with a smaller motion of the capsule.

It remains to be inquired, whether these fibres can produce an alteration in the form of the lens sufficiently great to account for the known effects.

In the ox's eye, the diameter of the crystalline is 700 thousandths of an inch, the axis of its anterior segment 225, of its posterior 350. In the atmosphere it collects parallel rays at the distance of 235 thousandths. From these data we find, by means of Smith's Optics, Art. 366, and a quadratic, that its ratio of refraction is as 10000 to 6574. Hauksbee makes it only as 10000 to 6832,7, but we cannot depend on his experiment, since he says that the image of the candle which he viewed was enlarged and distorted; a circumstance that he does not explain, but which was evidently occasioned by the greater density of the central parts. Supposing, with HAUKSBEE and others, the refraction of the aqueous and vitreous humours equal to that of water, viz. as 10000 to 7465, the ratio of refraction of the crystalline in the eye will be as 10000 to 8806, and it would collect parallel rays at the distance of 1226 thousandths of an inch: but the distance of the retina from the crystalline is 550 thousandths, and that of the anterior surface of the cornea 250; hence (by SMITH, Art. 367,) the focal distance of the cornea and aqueous humour alone must be 2329. Now, supposing the crystalline to assume a spherical form, its diameter will be 642 thousandths, and its focal distance in the eye 926. Then, disregarding the thickness of the cornea, we find (by SMITH, Art. 370,) that such an eye will collect those rays on the retina, which diverge from a point at the distance of 12 inches and 8 tenths. This is a greater change than is necessary for an ox's eye, for if it be supposed capable of distinct vision at a distance somewhat less than 12 inches, yet it probably is far short of being able to collect parallel rays. The human crystalline is susceptible of a much greater change of form.

The ciliary zone may admit of as much extension as this diminution of the diameter of the crystalline will require; and its elasticity will assist the cellular texture of the vitreous humour, and perhaps the gelatinous part of the crystalline, in restoring the indolent form.

It may be questioned whether the retina takes any part in supplying the lens with nerves; but, from the analogy of the olfactory and auditory nerves, it seems more reasonable to suppose that the optic nerve serves no other purpose than that of conveying sensation to the brain.

Although a strong light and close examination are required, in order to see the fibres of the crystalline in its intire state, yet their direction may be demonstrated, and their attachment shewn, without much difficulty. In a dead eye the tendons are discernible through the capsule, and sometimes the anterior ones even through the cornea and aqueous humour. When the crystalline falls, it very frequently separates as far as the centre into three portions, each having a tendon in its middle. If it be carefully stripped of its capsule, and the smart blast of a fine blow-pipe be applied close to its surface in different parts, it will be found to crack exactly in the direction of the fibres above described, and all these cracks will be stopped as soon as they reach either of the tendons. The application of a little ink to the crystalline is of great use in shewing the course of the fibres.

When first I observed the structure of the crystalline, I was not aware that its muscularity had ever been suspected. We have, however, seen that DESCARTES supposed it to be of this nature; but he seems to think that the accommodation of the eye to a small distance is principally performed by the elongation of the eye's axis. Indeed as a bell shakes a steeple, so must the coats of the eye be affected by any change in the crystalline; but the effect of this will be very inconsiderable; yet, as far as it does take place, it will co-operate with the other change.

But the laborious and accurate LEEUWENHOEK, by the help of his powerful microscopes, has described the course of the fibres of the crystalline, in a variety of animals; and he has even gone so far as to call it a muscle *; but no one has pur-

* Now if the cristaline humour (which I have sometimes called the crist. muscle)

sued the hint, and probably for this reason, that from examining only dried preparations, he has imagined that each coat consists of circumvolutions of a single fibre, and has intirely overlooked the attachment of the fibres to tendons; and if the fibres were continued into each other in the manner that he describes, the strict analogy to muscle would be lost, and their contraction could not have that effect on the figure of the lens, which is produced by help of the tendons. Yet notwithstanding neither he, nor any other physiologist, has attempted to explain the accommodation of the eye to different distances by means of these fibres, still much anatomical merit must be allowed to the faithful description, and elegant delineation, of the crystallines of various animals, which he has given in the Philosophical Transactions, Vol. XIV. p. 780, and Vol. XXIV. p. 1723. It appears, from his descriptions and figures, that the crystalline of hogs, dogs, and cats, resembles what I have observed in oxen, sheep, and horses; that in hares and rabbits, the tendons on each side are only two, meeting in a straight line in the axis; and that in whales they are five, radiated in the same manner as where there are three. It is evident that this variety will make no material difference in the action of the muscle. I have not yet had an opportunity of examining the human crystalline, but from its readily dividing into three parts, we may infer that it is similar to that of the ox. The crystalline in fishes being spherical, such a change as I attribute to the lens in quadrupeds cannot take place in that class of animals.

It has been observed that the central part of the crystalline

in our eyes, &c. Phil. Trans. Vol. XXIV. p. 1729.— *Crystallinum musculum, alias humorem crystallinum dictum, &c.* Leeuwenh. *op. omn.* I. p. 102.

becomes rigid by age, and this is sufficient to account for presbyopia, without any diminution of the humours; although I do not deny the existence of this diminution, as a concomitant circumstance.

I shall here beg leave to attempt the solution of some optical queries, which have not been much considered by authors.

1. MUSSCHENBROEK asks, What is the cause of the lateral radiations which seem to adhere to a candle viewed with winking eyes? I answer, the most conspicuous radiations are those which, diverging from below, form, each with a vertical line, an angle of about seven degrees; this angle is equal to that which the edges of the eyelids when closed make with a horizontal line; and the radiations are evidently caused by the reflection of light from those flattened edges. The lateral radiations are produced by the light reflected from the edges of the lateral parts of the pupillary margin of the uvea, while its superior and inferior portions are covered by the eyelids. The whole uvea being hidden before the total close of the eyelids, these horizontal radiations vanish before the perpendicular ones.

2. Some have inquired, Whence arises that luminous cross, which seems to proceed from the image of a candle in a looking-glass? This is produced by the direction of the friction by which the glass is polished: the scratches placed in a horizontal direction, exhibiting the perpendicular part of the cross, and the vertical scratches the horizontal part, in a manner that may easily be conceived.

3. Why do sparks appear to be emitted when the eye is rubbed or compressed in the dark? This is MUSSCHENBROEK'S

fourth query. When a broadish pressure, as that of the finger, is made on the opaque part of the eye in the dark, an orbicular spectrum appears on the part opposite to that which is pressed: the light of the disc is faint, that of the circumference much stronger; but when a narrow surface is applied, as that of a pin's head, or of the nail, the image is narrow and bright. This is evidently occasioned by the irritation of the retina at the part touched, referred by the mind to the place from whence light coming through the pupil would fall on this spot; the irritation is greatest where the flexure is greatest, viz. at the circumference, and sometimes at the centre, of the depressed part. But in the presence of light, whether the eye be open or closed, the circumference only will be luminous, and the disc dark; and if the eye be viewing any object at the part where the image appears, that object will be totally invisible. Hence it follows, that the tension and compression of the retina destroys all the irritation, except that which is produced by its flexure; and this is so slight on the disc, that the apparent light there is fainter than that of the rays arriving at all other parts through the eyelids. This experiment demonstrates a truth, which may be inferred from many other arguments, and is indeed almost an axiom, viz. that the supposed rectification of the inverted image on the retina does not depend on the direction of the incident rays. NEWTON, in his sixteenth query, has described this phantom as of pavonian colours, but I can distinguish no other than white; and it seems most natural that this, being the compound or average of all existing sensations of light, should be produced when nothing determines to any particular colour. This average seems to resemble the middle form, which Sir JOSHUA REYnolds has elegantly insisted on in his discourses; so that perhaps some principles of beautiful contrast of colours may be drawn from hence, it being probable that those colours which together approach near to white light will have the most pleasing effect in apposition. It must be observed, that the sensation of light from pressure of the eye subsides almost instantly after the motion of pressure has ceased, so that the cause of the irritation of the retina is a change, and not a difference, of form; and therefore the sensation of light appears to depend immediately on a minute motion of some part of the optic nerve.

If the anterior part of the eye be repeatedly pressed, so as to occasion some degree of pain, and a continued pressure be then made on the sclerotica, while an interrupted pressure is made on the cornea; we shall frequently be able to observe an appearance of luminous lines, branched, and somewhat connected with each other, darting from every part of the field of view, towards a centre a little exterior and superior to the axis of the eye. This centre corresponds to the insertion of the optic nerve, and the appearance of lines is probably occasioned by that motion of the retina which is produced by the sudden return of the circulating fluid, into the veins accompanying the ramifications of the arteria centralis, after having been detained by the pressure which is now intermitted. As such an obstruction and such a re-admission must require particular circumstances, in order to be effected in a sensible degree, it may naturally be supposed that this experiment will not always easily succeed.

Fig. 1.

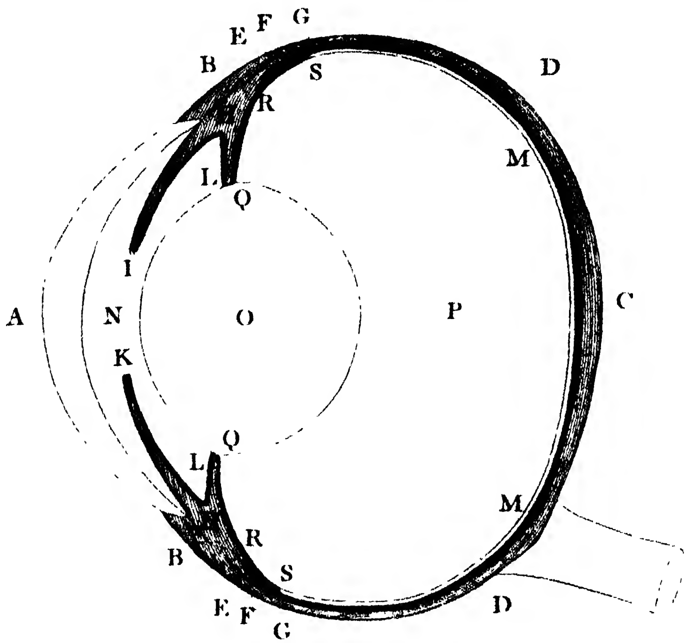


Fig. 2

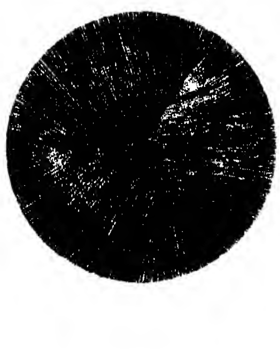
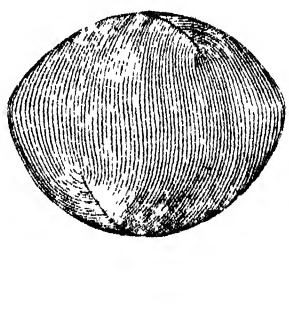


Fig. 3.



Explanation of the Figures.

Tab. XX. fig. 1. A vertical section of the ox's eye, of twice the natural size.

A. The cornea, covered by the tunica conjunctiva.

BCB. The sclerotica, covered at BB by the tunica albuginea, and tunica conjunctiva.

DD. The choroid, consisting of two laminas.

EE. The circle of adherence of the choroid and sclerotica.

FG, FG. The orbiculus ciliaris.

HI, HK. The uvea: its anterior surface the iris; its posterior surface lined with pigmentum nigrum.

IK. The pupil.

HL, HL. The ciliary processes, covered with pigmentum nigrum.

MM. The retina.

N. The aqueous humour.

O. The crystalline lens.

P. The vitreous humour.

QR, QR. The zona ciliaris.

RS, RS. The annulus mucosus.

Fig. 2. The structure of the crystalline lens, as viewed in front.

Fig. 3. A side view of the crystalline.

XVII. *Observations on a Current that often prevails to the Westward of* Scilly; *endangering the Safety of Ships that approach the* British *Channel. By* James Rennell, *Esq. F. R. S.*

Read June 6, 1793.

It is a circumstance well known to seamen, that ships, in coming from the Atlantic, and steering a course for the British channel, in a parallel somewhat to the *south* of the Scilly Islands; do, notwithstanding, often find themselves to the *north* of those islands: or, in other words, in the mouth of the St. George's, or of the Bristol channel. This extraordinary error has passed for the effects, either of bad steerage, bad observations of latitude, or the indraught of the Bristol channel: but none of these account for it satisfactorily; because, admitting that at times there may be an indraught, it cannot be supposed to extend to Scilly; and the case has happened in weather the most favourable for navigating, and for taking observations. The consequences of this deviation from the intended track, have very often been fatal: particularly in the loss of the Nancy packet, in our own times; and that of Sir Cloudesley Shovel, and others of his fleet, at the beginning of the present century. Numbers of cases, equally melancholy, but of less celebrity, have occurred; and many others, in which the danger has been imminent, but not fatal, have scarcely reached the public ear. All of these have been referred to accident; and therefore no attempt seems to have been made, to investigate the cause of them.

I am however of opinion, that they may be imputed to a specific cause; namely, a current: and I shall therefore endeavour to investigate both that, and its effects; that seamen may be apprized of the times, when they are particularly to expect it, in any considerable degree of strength; for then only, it is likely to occasion mischief; the current that prevails at ordinary times, being, probably, too weak to produce an error in the reckoning, equal to the difference of parallel, between the south part of Scilly, and the track that a commander, prudent in his measures, but unsuspicious of a current, would chuse to sail in.*

It seems to be generally allowed, that there is always a current, setting round the Capes of Finisterre, and Ortegal, into the Bay of Biscay. This I have the authority of Captain MENDOZA RIOS, a Fellow of the Royal Society, and an officer in the royal navy of Spain, for asserting. Besides, such an intimation was amongst the earliest notices that I received, concerning matters of navigation, when on board of a ship that sailed close along the north coast of Spain, in 1757. The current then, is admitted to set to the eastward, along the coast of Spain; and continues its course, as I am assured, along the coast of France, to the north, and north-west: and indeed, any body of water, once set in motion, along a coast, cannot suddenly stop; nor does it, probably, lose that motion, until

* It may be remarked, by the way, that the true latitude of the present light-house on St. Agnes's Island, is 49°, 54'; and that of the most southerly part of the whole group of islands and rocks, is 49°, 52'. This is according to an advertisement given out by the Trinity House, in 1792.

by degrees it mixes with the ocean; after being projected into it, either from the side of some promontory, that extends very far beyond the general direction of the coast; or after being conducted into it, through a strait.

The original cause of this current, I apprehend to be, the prevalence of westerly winds in the Atlantic; which, impelling the waters along the north coast of Spain, occasions a current, in the first instance. The stronger the wind, the more water will be driven into the Bay of Biscay, in a given time; and the longer the continuance of the wind, the farther will the vein of current extend.

It seems to be clearly proved, that currents of water, after running along a coast that suddenly changes its direction, (as happens on the French coast, at the promontory south of Brest) do not change their course with that of the shore, but preserve, for a considerable time, the direction which they received from the coast they last ran by. In some instances, after being projected into the sea, they *never* again approach the shore; but preserve, to a very great distance, nearly the direction in which they were projected; as well as a considerable degree of their original velocity, and temperature. The gulf stream (of Florida) is a wonderful instance of this kind; which, originating in a body of *pent-up* waters, in the Gulf of Mexico, is discharged with such velocity, through the Straits of Bahama, that its motion is traceable through the Atlantic, to the Bank of Newfoundland; and may possibly extend much farther. This being therefore the case, we can have no difficulty in conceiving, that the current of the Bay of Biscay continues its course, which may be about NW by W, from the coast of France, to the westward of Scilly and Ireland.

At ordinary times, its strength may not be great enough to preserve its line of direction, across the mouth of the British Channel; or, if it does preserve its direction, it may not have velocity enough to throw a ship so far out of her course, as to put her in danger. But, that a current prevails *generally*, there can be little doubt; and its degree of strength will be regulated by the state of the winds. After a long interval of moderate westerly gales, it may be hardly perceptible; for a very few miles of northing, in the 24 hours, will be referred to bad steerage, or some other kind of error: but after hard and continued gales from the western quarter, the current will be felt in a considerable degree of strength; and not only in the parallel of Scilly, but in that of the south-west coast of Ireland likewise.

Our observation of what passes in the most common waters, is sufficient to shew how easily a current may be induced, by the action of the wind, on the water contiguous to a bank, when the wind blows *along* it. In a canal of about four miles in length, the water was kept up *four inches* higher at one end, than at the other, by the mere action of the wind, along the canal. This was an experiment made, and reported to me, by my much lamented acquaintance, the late Mr. SMEATON. We know also, the effects of a strong south-west, or north-west, wind, on our own coasts: namely, that of raising very high tides in the British Channel, or in the Thames, and on the eastern coasts; as those winds respectively blow: because the water that is accumulated, cannot escape quick enough, by the Strait of Dover, to allow of the level being preserved. Also, that the Baltic is kept up *two feet* at least, by a strong NW wind of any continuance: and that the Caspian Sea is higher by *several* feet, at either end, as a strong northerly, or southerly, wind prevails. Therefore, as water pent up, in a situation from which it *cannot escape*, acquires a higher level, so, in a place where it *can escape*, the same operation produces a current: and this current will extend to a greater or less distance, according to the force with which it is set in motion; or, in other words, according to the height at which it is kept up, by the wind.

It may possibly be asked, why a similar current does not prevail in the British Channel, from the same westerly winds? To this I answer, that the increased height and velocity of the tides, during the prevalence of such winds, prove that a part, at least, of the same effect which happens in the Bay of Biscay, is produced in the Channel; and I have little doubt, that there is, in fact, a current also; but that, as it is blended with the common tide, the effect on the senses is lost: for it may appear only in the form of a *stronger* flood tide, or a *weaker* ebb, than at other times. Whereas the Bay, a wider space, and of a different form, allows a freer scope to the tides, than the British Channel does: it being high water nearly at the same time, all over the Bay; but varying in the Channel, at least five hours. And it may be concluded, from analogy, that the form of the Channel does not allow of the same effect being produced by the wind, on its included waters, as may be produced on those of the Bay: these meeting with an opposition, in the coast of France, the others having a partial exit, at the Strait of Dover: we may also conclude, that if no such phænomenon as a tide existed, a current, though less strong than in the Bay, would be perceived in the British Channel.

Of the Bay of Biscay it may be observed that, by reason of its form, and exposure to the reigning winds, which are often violent, and which pass over a vast expanse of water, there is no part of the ocean, familiarly known to us, whose circumstances are, in any degree, similar to it. It ought not therefore to surprize us, if we find that it differs, in any particular, from other seas. Seamen have remarked its uncommon degree of agitation, in stormy weather; but this has not, as far as I know, been properly accounted for. May it not be owing generally, to the same cause as that which produces the current? and at times, to the very current itself? With respect to the first—the waves of a deep bay or gulf, when the wind forces the water into it, will meet with a resistance in the land at the head of it, which must occasion a reverberation, that will render the surface of a great part of the gulf more unquiet, than when there is an opening at the end, to allow the undulatory motion a freer scope. What is said here, is exemplified on a small scale, by Mr. SMEATON's very ingenious manner of quieting Ramsgate harbour. (See his Tract on that harbour, page 45.) And with respect to the second cause—the effect of a current running to windward, in producing a short, hollow, and therefore dangerous, wave, is pretty well known. Accordingly, at seasons when the current runs strong, and the wind blows fresh from the north-west quarter, this cause must also contribute to the agitation of the waters, in the north part of the bay.*

* How far the reverberatory motion may extend, I know not: but it is certain that an undulatory motion impressed on the sea by the wind, will extend to a prodigious distance; and even into a region where a different wind prevails: as for instance, a swell raised by a strong gale, at south, or south-west, in the tract of variable winds,

It is quite uncertain at what interval of time, from the commencement of strong westerly gales, in the Atlantic and Bay of Biscay, the current may operate on the tracks of ships, near Scilly; for we are not possessed of the *data*, requisite for determining it. If we were to conceive a current, originating on the coast of Spain, and afterwards disturbing the courses of ships, on the west of Scilly and Ireland; this would require too much time, to agree with one of the instances which I mean to adduce: although it is probable, that this may be nearly the effect at ordinary times, and when the westerly winds blow moderately. But as, in one striking instance, it appears that the current operated in a very remarkable manner, on the ship's course, on the fourth day after the commencement of the gale, in the quarter where the ship was; the cause should rather be looked for, in the *sudden* and *great accumulation* of water, in the Bay of Biscay: otherwise, there is no accounting for the sudden appearance of the current. And the very act of accumulation, causing an indraught, there will consequently be a current round the Capes of Finisterre, and Ortegal, towards the Bay. Be the exact cause, however, what it may, it no doubt originates in the Bay, by the action of strong westerly winds: the prevalence of such winds, will therefore be the *signal* for the appearance of a current, between Ushant, and the south-west coast of Ireland: for though the cause can only be guessed at, the effect is too well ascertained, to remain in doubt.

I shall now adduce the facts, on which the idea of the existence of a current is founded.

has been felt, very far within the limits of the south-east trade wind, in the Indian Ocean.

In crossing the eastern part of the Atlantic, in the Hector East India ship, in 1778, we encountered, between the parallels of 42 and 49, very strong westerly gales; but particularly between the 16th and 24th of January, when, at intervals, it blew with uncommon violence. It varied two, or more, points, both to the north and south of west, but blew longest from the northern points; and it extended, as I afterwards learnt, from the coast of Nova Scotia, to that of Spain.

We arrived within 60 or 70 leagues of the meridian of Scilly, on the 30th of January, keeping between the parallels of 49 and 50; and about this time we began to feel a current, which set the ship to the north of her intended parallel, by near half a degree, in the interval between two observations of latitude; that is, in two days. And the wind, ever afterwards, inclining to the south, would not permit us to regain the parallel; for, although the northern *set* was trifling, from the 31st until we arrived very near Scilly; yet the wind, being both *scant* and *light*, we could never overcome the tendency of the current. Add to this, that the direction of the current, being much more *westerly* than *northerly*, we crossed it on so very oblique a course, that we continued in it a long time; and were driven, as it appears, near 30 leagues to the west, by it: for we had soundings in 73 fathoms, in the latitude of Scilly, and afterwards ran 150 miles, by the log, directly east, before we came the length of the islands. In effect, in running 120 miles, we shallowed the water, only nine fathoms.

We not only were sensible of the current, by the observations of latitude, but by *riplings* on the surface of the water, and by the direction of the lead line. The consequence of all this was, that we were driven to the north of Scilly; and were barely able to lay a course through the passage between those islands and the Land's End.

Having no time keeper on board, we were unable to ascertain the several points, in this part of our track, and therefore can only approximate our longitude; and that but very coarsely. But according to what we learnt from our soundings, and from a vessel which had only just entered the current, it may be concluded, that the current, at times, extends to 60 leagues, west of Scilly; and also runs close on the west of those islands. However, the breadth of the stream, may probably be little more than 30 leagues; for we crossed it, as has been said, very obliquely; and perhaps, in the widest part.

The journal of the ATLAS East India ship, Captain COOPER, in 1787, furnishes much clearer proofs, both of the existence of the current, and of the rate of its motion: for having time keepers on board, Captain COOPER was frequently enabled to note the difference between the true, and the supposed, longitude; and it may be said, that this journal, by the means it affords of ascertaining the current, is highly valuable; as containing some very important facts, and which might have been entirely lost to the public, had not Captain COOPER marked them, in the most pointed manner.

I shall proceed to state, in abstract, the most important of the facts recorded in the journal.

The ATLAS sailed with a fair wind, and took her departure from the Isle of Wight, on the 25th of January, 1787; and on the 27th had advanced 55 leagues to the westward of Ushant; when a violent gale of wind began at south, and, about 11 hours afterwards, changed suddenly to the westward. The gale continued through the four following days: on the 28th, it was generally W by S, and WSW; on the 29th, SW by W, or more southerly; and on the 30th and 31st, SSW, to SW by S.*

During this long interval, the ship was generally *lying to;* and with her head to the NW. On the 1st of February, the wind abated, but still blew from the south-westward; and the ship was kept to the north-west. The stormy weather returned again the following day, and continued, with little intermission, until the 11th; blowing from all the intermediate points, between south and WNW; but chiefly, and most violently, from the WSW, and SW. At intervals, on the 8th and 9th in particular, the journal remarks, that "*it blew a mere hurricane.*" On the 11th, the weather growing more moderate, and the wind favourable, the ship proceeded on her course, southward; being then two degrees and a quarter of longitude, to the west of Cape Finisterre, by the reckoning; but by the time keepers, more than *four degrees and a half.*

After the above abstract of the proceedings of the ship, I shall subjoin the following particulars; which are the most in point, to the purpose of the present discussion.

On the 27th, at noon, soon after the gale commenced, the longitude, by reckoning, agreed within 14 minutes of that shewn by the time keepers; the latter being the most westerly. This difference alone might well have arisen from

* In this, as well as in the former statement of the winds, I have allowed for the variation of the compass; that the application of it, to the quarter of the heavens, and to the chart annexed, (see Tab. XXI.) may be more easy and clear.

an error in the log, or even in the position of the needle point on the Isle of Wight, from whence the departure was taken; but it may also be owing to the westerly current, whilst the ship remained in it, on the 27th; if we admit that such a current prevails at all times, though in different degrees of strength. Here it is proper to remark, that in delineating Captain COOPER's track, on the chart, I have scrupulously adhered to the result of each day's work, of the reckoning, as I find it in his journal; contenting myself with inserting my own observations on the track, in this paper only; where they cannot mislead.

The longitudes pointed out by the time keepers on the 28th, 29th, and 30th, shew, that the increasing, though trifling differences, between the true longitude, and that by the dead reckoning, had amounted to 24 minutes only, on the 30th. At this time the ship was about 24 leagues to the WSW of Scilly; and, at 5 or 6 leagues to the SSE of this position, (that is, at 25 leagues SW by W from Scilly) they had soundings at 70 fathoms. This last particular is mentioned, to prove that the longitude shewn by the time keepers (8° 28′ west from London) was nearly the longitude in which the ship really was, on the 30th of January. That of St. Agnes (Scilly) is taken at 6° 46′.

The Atlas was now entered into the stream of the same current which occasioned so much delay to the Hector; but the course of the Atlas, being opposite to that of the Hector, it facilitated her progress; and also carried her clear of the south-west coast of Ireland.

On the 31st, the time keepers shewed that the ship had been *set* very considerably to the westward of the reckoning; and by the 2d of February, at 3 in the afternoon, it appeared that she had been *set* two whole degrees of longitude to the west of the reckoning, since the 30th at noon; that is, in the course of 51 hours. (Here it may be proper to remark, that I have, throughout, reckoned according to *sea time;* that is, the day commences at noon.)

On the 3d of February, at noon, the time keepers shewed a further *set*, of 23 minutes of longitude, more than the reckoning gave, in the interval since the last observation, which was 45 hours; so that, since the 30th of January, 4 days only, the ship had been carried by the current, no less than two degrees and twenty-three minutes; and since the 27th, when the gale began, 2° 32′ of longitude; amounting, in these parallels, to ninety-nine marine miles. But here, the current appears to have totally left them; and it is very probable, that it even ceased before the time of observation, on the 3d: for the succeeding observations of the 5th, 6th, 7th, 9th, 10th, and 11th, although the strong westerly gales continued, come so near the longitude by the reckoning (deduced from the observation of the 3d) that the differences, which are sometimes to the east, and at other times to the west, may be with more propriety ascribed to errors of the log, than to a current; as may be seen by the two tracks on the chart. We may therefore conclude, that the current did not cease at the very point of time, when the observation of the 3d was taken, but probably some time before.

It appears then, that the Atlas experienced a westerly current, from a point about 24 leagues to the WSW of Scilly, (if not earlier) to four degrees of longitude west of the meridian of Cape Clear,* in the parallel of 51°; where its effects were no longer perceptible. And, as no current was felt in the track southward, on the 11th; nor in any part of the track to the north-west, between the 3d and 10th; although it was felt nearly in the same line of direction, between the 1st and 3d; it may be inferred that the stream goes off to the north-west, between the aforesaid track, and the south-west coast of Ireland. It is much to be regretted that no observations appear on the 12th and 13th; which would have been decisive of its course.

I come now to two particulars of the case, which, I confess, perplex me exceedingly. The first is, that the current was felt, apparently in its full strength, on the fourth day after the commencement of the gale; which began at south, then changed suddenly to the west and WSW, and afterwards fixed in the SW quarter. This gale was felt between the 48th and 50th degrees of latitude, and, no doubt, extended its effects very far to the south and west; but what the state of the winds had been in those quarters, previous to the 27th of January, we are ignorant. The winds in the British Channel had been easterly, for three days preceding the gale: the fourth day, preceding, there had been strong gales at SW; and the five days preceding *that*, there had been chiefly light winds at west. According to this state of facts, we can only suppose that the current originated from a vast body of water, pent up in the Bay of Biscay, by violent gales of wind; first from the southward, eleven hours; then from a point or two to the south of west; and lastly, at south-west.

* Cape Clear is reckoned to be in long. 9° 25′ from London.

We are not to consider the water of this current, as having made the circuit of the Bay of Biscay; but as the *collective body* of *pent up* waters, in the Bay, running off along the SW coast of Britanny, and thence to the north-westward; preserving nearly the direction it had acquired, by running along that coast. And it may be conceived, that the frequent recurrence of westerly winds, keeps up a constant current in the Bay, and to some distance beyond it; although during the longest intermissions of these winds, the current may become so slow, as to be scarce perceptible.

The second particular which perplexes me, is, that no northern *set* is indicated by Captain COOPER's journal: that is to say, by the mode in which each day's log is wrought; and which, in the formation of the chart, as is said before, I have strictly adhered to. It indeed appears to me very wonderful, that no northing should appear, when it seems to be the very same kind of current which carried the Hector so far to the northward. It is certain, that the state of the weather was such, as to preclude those nice attentions to the reckoning, which might enable us to detect any small differences, between the latitude by account, and that by observation; although the western *set* was too considerable to escape notice, and may even have been more than the statement sets forth. I cannot therefore, by any means admit, that there was no northing in the current through which the Atlas passed; first, because they had not observations of latitude, regularly; and lastly, because on the 31st of January, when *lying to*, 36 miles are allowed for 20 hours *drift*, to the north-west; which appears to me excessive. On that day they had no observation of latitude, and on the following day, the observation shewed two miles northing; which however proves nothing. Again, on the succeeding day, (the 2d) in a most important point of the track, there was no observation of latitude.

In the Hector, precisely in the same track, and at the same season of the year, the current had, as has been observed, a considerable degree of northing in its course. On two days it was about 12 miles, each; on another day 13, and on two others, 9, and 8; and this, in weather very favourable for keeping a reckoning, and with observations of latitude, on every day save one; not to mention the strong circumstances of a visible *set* to the northward, indicated, as well by the lead line, as by the ripling on the surface of the water. It is in the nature of currents, to expand their streams or columns of water, after being projected into the ocean; and therefore, according to this law, the middle part of the stream should preserve its original course, in a greater degree than the borders of it; so that the middle part may run to the NW by W, whilst the eastern border may run more *northerly*, and the western border more *westerly*. It is certain, that in the Hector, we felt the northerly current much stronger, close on the west of Scilly, than further out; and it appeared by the distance we ran, after sounding in 73 fathoms, that the current must have set much more *westerly*, than *northerly*, the whole time.

The following remarks obviously occur, on the effect of this current.

1st. Whatever may be the breadth of the stream, (which is at present unknown) if a ship crosses it *very obliquely*, that is, in an E by S, or more southerly direction (as may easily happen, on finding herself too far to the northward, at the first place of observation, after she gets into the current), she will, of course, continue much longer in it, and will be more affected by it, than if she steered more directly across it. She will be in a similar situation, if she crosses it with light winds; and both of these circumstances should be attended to. And if it be true, as I suspect it is, that the eastern border of the current has a more northerly direction than the middle of it, this also should be guarded against. I conceive also, that the stream is broader in the parallel of Scilly, than farther south. And here we may remark, that those who, from a parallel south of Scilly, have been carried clear of it to the north, when approaching it, in the night, may esteem themselves fortunate that the current was *so strong*; for had it been weaker, they might have been carried on the rocks.

2d. A good observation of latitude, at noon, would be thought a sufficient warrant for running eastward, during *a long night*: yet as it may be possible to remain in the current, long enough to be carried from a parallel that may be deemed a very safe one, to that of the rocks of Scilly, in the course of such a night; it would appear prudent, after experiencing a continuance of strong westerly gales in the Atlantic, and approaching the Channel with light southerly winds, either to make Ushant, or at all events to keep in the parallel of 48°, 45′, at the highest. If they keep in 49°, 30′, they will experience the whole effect of the current, in a position where they can least remedy the evil: but if in 48°, 45′, they are assailed by the north-west current, they are still in a position from whence a southerly wind will carry them into the

Channel. But all ships that cross the Atlantic, and are bound to the eastward of the Lizard, had better make Ushant, under the above circumstances, in times of peace. Or, at all events, why should they run in a parallel, in which they are likely to lose ground?

3d. Ships, bound to the westward, from the mouth of the Channel, with the wind in the south-west quarter, so that it may appear indifferent which tack they go on, should prefer the *larboard* tack; as they will then have the benefit of the current.

4th. I understand that the light house of Scilly is either removed, or to be removed, to the south-west part of the islands; or of the high rocks. This is certainly a wise measure; as the light should be calculated more particularly for ships that have a *long*, than a *short* departure; like those from any part of the European coasts, to the northward, or eastward. The light house ought also to be built very lofty. I am sorry to remark, that, as far as my observation has gone, this light has never appeared clear and bright, as a light to direct ships ought to do.

5th. It would be worth the attention of government (in my humble opinion) to send a vessel with time keepers on board, in order to examine and note the soundings between the parallels of Scilly and Ushant, at least; from the meridian of the Lizard point, as far west as the moderate depths extend; I mean such as can be ascertained with exactness, in the ordinary method of sounding. I have reason to suppose that our chart of soundings is very bad; and indeed, how can it be otherwise, considering the imperfect state of the art of marine surveying, at the time when it was made? A set of time keepers will effect more, in the course of a summer, in the hands of a skilful practitioner, than all the science of Dr. Halley, during a long life; for who could place a single cast of soundings, in the open sea, without the aid of a time keeper? The current in question, must have disturbed every operation of this kind. It should be the task of the person, so employed, to note all the varieties of bottom, as well as the depths; the time of high and low water; setting of the tides, and currents, &c. Such a survey, skilfully conducted, might enable mariners to supply the want of observations of latitude, and of longitude; and, of course, to defy the current, as far as relates to its power of misleading them.

6th. It is certain, that the current in question may be somewhat disturbed by, or rather will appear to be blended with, the tides, at the entrances of the British and St. George's Channels; but it is obvious that the current will have the same effect, in setting a ship out of her course, as if no tide existed; because, whatever effect one tide may have, the next will nearly do away. But there are two particulars, well worth ascertaining; and these are, first, the point at which the two tides of St. George's, and of the British Channel separate, on the west of Scilly. And secondly, what degree of northing one of the streams has, more than the other. Because a ship, in approaching Scilly, from the west, on a flood tide, and keeping in a parallel which may be to the *north* of the point of separation of the two tides, (and consequently in the tide stream of St. George's Channel) may be thrown too far to the north; although, had she been far enough to the west, to receive the effect of the next ebb, this temporary, and alternate derangement of the course, would have had no ill effect; or even have been noticed. But admitting that a tide, with any degree of northing in it, does take place, a little to the west of Scilly; this will furnish an additional reason for keeping in a southern parallel.

XVIII. *Observations on the Planet Venus.* By William Herschel, *LL. D. F. R. S.*

Read June 13, 1793.

THE planet Venus is an object that has long engaged my particular attention. A series of observations upon it, which I began in April, 1777, has been continued down to the present time.

My first view, when I engaged in the pursuit, was to ascertain the diurnal rotation of this planet; which, from the contradictory accounts of CASSINI and BIANCHINI, the former of which states it at 23 hours, while the latter makes it 24 days, appeared to me to remain unknown, as to its real duration: for the observations of these gentlemen, how widely different soever with regard to time, can leave no doubt but that this planet actually has a motion on its axis.

The next object was the atmosphere of Venus; of the existence of which also, after a few months observations, I could not entertain the least doubt.

The investigation of the real diameter, was the third object I had in view.

To which may be added, in the last place, an attention to the construction of the planet, with regard to permanent appearances; such as might be occasioned by, or ascribed to, seas, continents, or mountains.

The result of my observations would have been communicated long ago, if I had not still flattered myself with the hopes of some better success, concerning the diurnal motion of Venus; which, on account of the density of the atmosphere of this planet, has still eluded my constant attention, as far as concerns its period and direction. Even at this present time, I should hesitate to give the following extract from my journals, if it did not seem incumbent upon me to examine by what accident I came to overlook mountains in this planet, which are said to be "*of such enormous height, as to exceed four, five, and even six times the perpendicular elevation of Cimboraco, the highest of our mountains!*"*

The same paper, which contains the lines I have quoted, gives us likewise many extraordinary relations, equally wonderful; such as hints of the various and singular properties of the atmosphere of Saturn.† A ragged margin in Venus, resembling the uneven border of the moon, as it appears to a power magnifying from 1 to 4.‡ One cusp of Venus appearing pointed, and the other blunt, owing to the shadow of some mountain.§ Flat spherical forms conspicuous on Saturn.|| All which being things of which I have never taken any notice, it will not be amiss to shew, by what follows, that neither want of attention, nor a deficiency of instruments, could occasion my not perceiving *these mountains of more than 23 miles in height*;** *this jagged border of Venus*; *and these flat spherical forms on Saturn.*

* See Phil. Trans. for 1792, Part II. page 337. † Ibidem, p. 309. ‡ p. 310. § p. 312. || p. 336. ** The height of Chimbo-raço, according to Mr. CONDAMINE, is 3200 French toises; and the English mile, by Mr. DE LA LANDE, measures 830. If the mountains in Venus exceed Chimbo-raço six times in perpendicular elevation, they must be more than 23 miles in height.

Indeed with regard to Saturn, I cannot hesitate a single moment to say, that, had any such things as flat spherical forms existed, they could not possibly have escaped my notice, in the numberless observations with 7, 10, 20, and 40-feet reflectors, which I have so often directed to that planet. However, if the gentleman who has seen the mountains in Venus, has made observations on flat spherical forms on Saturn, it is to be regretted that he has not attended to the revolution of this planet on its axis, which could not remain an hour unknown to him when he saw these forms.

Last night,* for instance, I saw two small dark spots on Jupiter; I shall not call them flat spherical forms, because their flatness, as well as their sphericity, must be hypothetical; moreover, these two terms seem to me to contradict each other. These were evidently removed, in less than an hour, in such a manner as to point out, very nearly, the direction and quantity of the rotation of this planet.

Before I remark on the rest of the extraordinary relations above-mentioned, I will give a short extract of my observations on Venus, with such deductions as it seems to me that we are authorised to make from them.

Observations.

April 17, 1777. The disk of Venus was exceedingly well defined, distinct, and bright, but no spot was visible by which I could judge of her diurnal motion. The same telescope shews the spots on Mars extremely well. 7-feet reflector.

April 26, 1777. The disk well defined, and bright, but no spot. 10-feet reflector.

* May 31, 1793.

February 21, 1780. No spot on the disk of Venus; diameter 15″,9, mean of three measures.

May 2, 1780. No spot; power 449; diameter 17″,2.

May 28, 1780. No spot; power 268 and 449; diameter 22″,8.

May 29, 1780. I viewed Venus with a 20-feet Newtonian reflector; power 447. The edge of the disk was so sharp and well defined, that there can be no wish to see it better. There was no spot of any kind.

I could see no projections of any mountains, though the phase of Venus is now such as would be most favourable for shewing them.

June 19, 1780. There is, on Venus, a bluish, darkish spot, *a d c;* and another, which is rather bright, *c e d;* they meet in an angle at *c*, the place of which is about one-third of the diameter of Venus from the cusp *a*. See Tab. XXII. fig. 1.

June 21, 23, 24, 25, 26, 28, 29, 30, and July 3, 1780. Continued observations were made upon these, and other faint spots, and drawings of them annexed. The instrument I used was a 20-feet Newtonian reflector, furnished with no less than five different object specula, some of which were in the highest perfection of figure and polish; the power generally 300 and 450. But the result of them would not give me the time of the rotation of Venus. For the spots assumed often the appearances of optical deceptions, such as might arise from prismatic affections; and I was always very unwilling to lay any stress upon the motion of spots, that either were extremely faint and changeable, or whose situation could not be precisely ascertained.

However, that Venus has a motion on an axis cannot be doubted from these observations; and that she has an atmosphere is as evident, from the changes I took notice of, which surely cannot be upon the solid body of the planet.

Sept. 18, 1780. No spot on Venus; diameter 38″,4.

Oct. 10, 1780. With a very perfect 7-feet speculum; power 227, 460, and 932. No spot visible; diameter 41″,3.

Oct. 11, 1780. No spot; diameter 27″,8.

Oct. 20, 21, 23, 1780. No spot visible.

April 17, 1783. 10-feet reflector; power 324. I see some darkish spots on Venus. 7-feet reflector; power 227. The same appearances; but in neither of the instruments are they determined enough to serve for the purpose of finding the rotation.

May 21, 1783. 10-feet reflector; a new speculum; power 250.

7^h 30′. No spot visible.

8^h 30′. There seems to be an ill defined spot.

9^h 15′. No motion can be perceived that may be depended upon, though the figure seems rather advancing towards the centre.

May 30, 31, and June 1, 6, 1783. Spots were observed with 10 and 20-feet reflectors, and also motion perceived in them. Continued observations were recorded; and a great many figures delineated.

Dec. 3, 1783. With 460, and 932. No spot. No kind of protuberance, or indenture in the line which terminates the illumination, that might denote a mountain.

Feb. 13, 1785. No spot. A new 10-feet Newtonian reflector.

April 8, 1788. No spot on Venus; but she is still at too great a distance for such observations.

Nov. 30, 1789. No satellite visible. If she has one, it must be less in appearance than a star of the 8th or 9th magnitude; power 300.

Dec. 2, 1789. No spot; power 157, 300, and 460.

May 23, 1791. 40-feet reflector. The light of Venus is so brilliant that it becomes very uneasy for the eye to bear it long. There is no spot on the disk.

I had prepared my apparatus for a regular succession of observations with this instrument, having turned it towards the west, and put on the round-motion to keep the planet in view; but found that the great advantage of this telescope, which is its superior light, was, on this occasion, not only unnecessary, but rather an inconvenience.

Nov. 24, 1791. Correction of the clock, —46",7.

I took measures of the diameter of Venus with the 20-feet reflector; power 157.

12h 18'	1st measure	45",486
	2d ———	46 ,142
	3d ———	45 ,514
	4th ———	45 ,814
	5th ———	46 ,033
	6th ———	46 ,252

Mean of the six measures 45",874

I took five more, with a power of 300, the morning being very fine and clear.

12h 36′	1st measure		44″,885
	2d	———	45 ,705
	3d	———	45 ,104
	4th	———	45 ,322
	5th	———	45 ,842

Mean of the five measures 45″,372

Mean of the two sets 45″,623

These measures were taken with a speculum that has been lately re-polished, and therefore required new tables for casting them up. Such tables were made by the following transits.

Nov. 25, 1791. Transits of equatorial stars, taken to determine the value of the micrometer, which is divided into revolutions of sixty parts each.

First set, 23″,0 23,0 23,0 23,0 23,2 23,1 23,1 23,0 23,1 23,1 = 23″,06 = 21 revolutions; correction + 7,2 parts, for zero and concave wires.

Second set, 16″,8 16,6 16,4 16,5 16,7 16,6 16,5 16,8 16,4 16,5 = 16,58 = 15 revolutions 1,3 parts. Correction + 7,2.

By the first set, 1 part = 0″,272964

Second set - - 273748

Mean of the two sets 0″,273356

In the first set, the micrometer was opened to 21 revolutions; and ten equatorial stars were observed to pass from one wire to the other. The opening was afterwards changed, and ten other stars were again observed to pass over the wires; after which the micrometer was read off, and found to be 15 revolutions and 1,3 parts.

Feb. 4, 1793. Correction of the clock, — 1′ 28″,0.

2^h 55′. 7-feet reflector; power 172. The air is very clear, and I see Venus very well defined; but cannot perceive any inequality on the edge of the planet that might denote a mountain; though the situation is favourable, being a little more enlightened than what we may call her last quarter. With 215, I had a very distinct view for a long time; but cannot perceive any inequality on the line which divides light from darkness.

With 287, I perceive no mountains: with 430, very distinct, I perceive no mountains. The terminating line is not so sharply defined as the circumference; but no inequality is visible.

With the same power, I see on Saturn, the equatorial belt, the shadow of the ring on Saturn, the shadow of Saturn on the ring, the division of the ring, &c.

I do not find any spot on Venus; so that there is no possibility to assign its diurnal motion.

March 3, 1793. Correction of the clock, — 2′ 0″,6.

6^h 30′. 7-feet reflector; I observed Venus with many powers, but could perceive no spot by which its diurnal motion might be ascertained.

April 3, 1793. Correction of the clock, — 2′ 43″,9.

9^h 9′. 7-feet reflector; power 215. The evening remarkably fine. There is no spot upon the disk of Venus, by which its rotation might be ascertained. The horns are equally sharp. There is nothing that has the appearance of a mountain, like what we see in the moon. With 287, very well defined, appearances are the same. With 430, not the least appearance of any mountains.

April 4, 1793. Correction of the clock, — 2′ 45″,8.

9^h 8′. There is no spot upon the disk of Venus. The horns are perfectly alike.

Not the least appearance like the mountains of the moon. With 287, and 430, very distinct.

April 5, 1793. Correction of the clock, — 2′ 46″,7.

8^h 25′. 7-feet reflector; power 215, 287, and 430. There are no spots upon Venus, by which its diurnal motion could be ascertained. The horns are exactly alike; and no inequality, like the mountains of the moon, is visible.

April 6, 1793. Correction of the clock, — 2′ 48″,1.

9^h 29′. With the 7-feet reflector; power 430. There is no kind of spot visible in any part of the disk. The two horns are exactly alike; and no appearance of mountains can be perceived.

April 7, 1793. Correction of the clock, — 2′ 49″,6.

9^h 8′. With the 7-feet reflector; power 215, 287, 430, and 860. I can see no spot upon the disk. Both horns are perfectly alike. Nothing resembling the mountains upon the moon can be perceived. I see it beautifully well, and sharply defined.

April 8, 1793. Correction of the clock, — 2′ 51″,0.

9^h 2′. With the 10-feet reflector; power 300, and 400. There is no spot upon Venus. The shape of the two horns is perfectly alike, and no appearance of mountains can be perceived. The illumination of the horns is also perfectly alike.

April 9, 1793. Correction of the clock, — 2′ 52″,1.

8^h 45′. With the 10-feet reflector; power 300. No spot upon Venus. Both horns perfectly alike. No appearance of mountains.

The light of Venus is brighter all around the limb, than on that part which divides the enlightened, from the unenlightened part of the disk. With 400, appearances are the same.

9^h 16′. The bright part, on the limb of Venus, is like a bright bead, of nearly an equal breadth all around.

April 16, 1793. Correction of the clock, — 2′ 59″,5.

10^h 3′. 7-feet reflector, with different powers. No spot upon the disk. No mountains visible. Both horns alike.

A luminous margin, as usual, all around the limb.

April 20, 1793. Correction of the clock, — 3′ 3″,8.

10^h 0′. 7-feet reflector; power 172, 215, 287, 430, and 860. No spot upon the disk. Both horns exactly alike. Not the least appearance of any mountains.

With 287, there is a narrow luminous border all around the limb, and the light afterwards diminishes pretty suddenly, and suffers no considerable diminution as we go towards the line which terminates the enlightened part of the disk. It is however less bright near the terminating line than farther from it. With powers lower than 287, the narrow luminous border cannot be so well distinguished.

April 22, 1793. Correction of the clock, — 3′ 5″,9.

9^h 30′. 7-feet reflector; power 430. Very distinct. No spot. No appearance of mountains. Both horns perfectly alike.

With 860, 1290, and 1720, not the least appearance of mountains. Even the last power is considerably distinct.

10^h 20′. With 430, the luminous margin, compared to the light adjoining to it, may be expressed by, *suddenly much brighter all around the limb.*

April 28, 1793. Correction of the clock, — 3′ 12″,3.

12^h 0′. 7-feet reflector; power 215. No spot. Both horns perfectly alike. No appearance of mountains.

April 29, 1793. Correction of the clock, — 3′ 13″,4.

10^h 30′. 7-feet reflector; power 215. No spot. Both horns perfectly alike. Not the least appearance of any mountains.

With 287 and 430. Both horns equally sharp: no mountains visible.

May 1, 1793. Correction of the clock, — 3′ 15″,5.

10^h 45′. With the 10-feet reflector; power 300. No spot. Both horns perfectly alike, and very sharp. Not the least appearance of any mountains.

With 600, very distinct. Both horns extremely sharp, and alike. No mountains.

With 400, the same appearances.

May 5, 1793. Correction of the clock, — 3′ 19″,8.

11^h 27′. 7-feet reflector; power 215, 287, and 430. Both horns perfectly alike. No spot. Not the least appearance of any mountains.

May 12, 1793. Correction of the clock, — 3′ 27″,3.

11^h 10′. 7-feet reflector; power 215. Beautifully distinct. No spot visible; indeed the crescent is so slender, that we cannot expect to see any spots upon the disk.

Not the least appearance of any mountains, or inequality on the border.

The slender part of the crescent appears often knotty, but this is evidently a deception arising from undulations in the air; for, with proper attention, the knots may be perceived to change place. Little scratches in the great, or small speculum, may also occasion seeming irregularities; but, with proper attention, all such deceptions may be easily detected. Both horns perfectly alike.

With 287, 430, and 860, all that has been mentioned before is perfectly verified, and confirmed.

11^h 43′. I tried also the lower powers of 172, and 115; but they are inferior, in effect, to 215, 287, and 430; and not adequate to the delicacy and power required in such observations.

I have often taken notice, and again this evening, that the illuminated part of Venus is more than a semi-circle. Whether the excess of the sun's diameter alone will account for this, or how far we are to take the twilight of the atmosphere of Venus into consideration, I have hitherto deferred investigating, as my disk-micrometer wants a moveable parallel, in order to be adjustable, by observation, to the quantity of the horns which is enlightened beyond an hemisphere.

May 13, 1793. Correction of the clock, — 3′ 28″,4.

11^h 45′. 7-feet reflector; power 115, 172, 215, 287, and 430. Both horns perfectly alike. No appearance of mountains.

The points of the horns appear more blunt than they were last night, and are not drawn out to so slender a point; but this is evidently a deception, owing to the indifference of the night; for great sharpness, and distinct vision, are wanting in every other object I am looking at.

May 18. 1793. Correction of the clock, — 3′ 33″,7.

12^h 28′. 7-feet reflector; power 287. Both horns perfectly alike. No appearance of mountains. No spot. But, at the present altitude of Venus, it is impossible to make any observations that require delicacy, and demand very distinct vision with high powers.

May 19, 1793. Correction of the clock, — 3′ 34″,7.

11^h 45′. 7-feet reflector; power 287. Both horns perfectly alike, in shape and illumination. Not the least appearance of any mountains. The horns are exceedingly slender.

12^h 0′. I do not see any diminution of light on the edge of the horns, but what may be accounted for from their slenderness; being brought to very fine points, that lose themselves by their minuteness.

I saw it in great perfection, with a newly polished, plain speculum, which excels my former one in sharpness.

May 20, 1793. Correction of the clock, — 3′ 35″,8.

12^h 20′. No spot or unevenness in the light of Venus upon either cusp, or in any other part, that could in the least make me suspect a mountain.

I measured the diameter of Venus, and projection of the cusps beyond an hemisphere, by my disk-micrometer. This was not done by an illumination, as described in the apparatus, (Phil. Trans. Vol. LXXIII. p. 4.) when I used it for a nocturnal planet; for, day-light being sufficiently strong, there was no occasion to light the lamps. On the measuring disk were drawn concentric circles; and also a diameter, having several lines parallel to it, in one of the semicircles. If there had been time, I should have prepared a straight edge, *b e*, moveable parallel to the diameter *a d*. See Tab. XXII. fig. 2.

First measure, with the double eye-glass; power about 90. Diameter of Venus 2390. Projection 500. But the power is too low to be accurate.

Second measure; power 215. Diameter of Venus 4800. Projection 620. Here the projection is probably as much too small as the former was too large; but the planet is too low for repeating the measures. A mean of both may, perhaps, not be far from the truth; which gives, diameter 3595; projection 560.

Here 1797,5 being radius, and 560 sine, we find the angle *a c b*, or *d c e*, equal to 18° 9′ 8″,2.

A few very evident results may be drawn from the foregoing observations.

With regard to the rotation of Venus on an axis, it appears that we may be assured of this planet's having a diurnal motion, and though the real time of it is still subject to considerable doubts, it can hardly be so slow as 24 days. Its direction, or rather the position of the axis of Venus, is involved in still greater uncertainty.

The atmosphere of Venus is probably very considerable; which appears not only from the changes that have been observed in the faint spots on its surface, but may also be inferred from the illumination of the cusps, when this planet is near its inferior conjunction; where the enlightened ends of the horns reach far beyond a semicircle. I must here take notice, that the author we have before quoted on this subject, has the merit of being the first who has pointed out this inference, but he has overlooked the penumbra arising from the diameter of the sun;* which has certainly a considerable share in

* He mentions it upon another occasion, and says in a note, p. 313, that " *this whole penumbra, which, according to the greatest apparent diameter of Venus, extends from* 59 *to* 60″, (for what reason he fixes upon these quantities does not appear) " *measures, in the direction perpendicular to the line of the cusps, only* 0″,36." But if, according to him, the apparent diameter of the sun be 44′, (which is less than it ought to be) the penumbra must certainly extend likewise upon the surface of Venus

the effect of the extended illumination, and in his angle of 15° 19′ will amount to more than two degrees and a third. His measures are also defective; as probably the mirror of his 7-feet reflector, which was a very excellent one, was by that time considerably tarnished, and had lost much of the light necessary to shew the extent of the cusps in their full brilliancy.

I do not give the calculations I have made of the extent of the twilight of Venus, because my measures were not so satisfactory to myself as I wish them to be; nor so near the conjunction as we may hereafter obtain them; neither were they sufficiently repeated. My computations, however, when compared to those given in the paper on the atmosphere of Venus, shew sufficiently that it is of much greater extent, or refractive power, than has been computed in that paper. Those calculations indeed are so full of inaccuracies, that it would be necessary to go over them again, in order to compare them strictly with my own, for which at present there is no leisure.

I ought also to take notice here, that the same author, it seems, has taken measures of the horns of Venus by an instrument, which, in his publications, he calls a *projection table*, and describes as his own *; of which, however, those who do not know its construction may have a very perfect idea, when they read the descriptions of my lamp, disk, and periphery

over 44′ of a great circle; and, in the situation which he mentions, that is, perpendicular to the line of the cusps at the time of the greatest elongation, and when the apparent diameter of Venus is 60″, (as he makes it) it must measure 0″,384.

* See *Beiträge zu den neuesten astronomischen Entdeckungen*, p. 210. And *Selenotopographische Fragmente*, p. 63.

micrometers, joined to what I have mentioned above, of using the disk-micrometer without lamps when day-light is sufficiently strong; or even with an illumination in front, where the object is bright enough to allow of it, such as the moon, &c.

I remember drawing the picture of a cottage by it, in the year 1776, which was at three or four miles distance; and going afterwards to compare the parts of it with the building, found them very justly delineated.

I have also many times had the honour of shewing my friends the accuracy of the method of applying one eye to the telescope, and the other to the projected picture of the object in view; by desiring them to make two points, with a pin, upon a card fixed up at a convenient place, where it might be viewed in my telescope; and this being done, I took the distance of these points from the picture I saw projected, in a pair of proportional compasses, one side of which was to the other as the distance of the object, divided by the distance of the image, to the magnifying power of the telescope; and giving the compasses to my friends, they generally found that the proportional ends of them exactly fitted the points they had made on the card. All which experiments are only so many different ways of using the lamp-micrometer.

As to the mountains in Venus, I may venture to say that no eye, which is not considerably better than mine, or assisted by much better instruments, will ever get a sight of them; though, from the analogy that obtains between the only two planetary globes we can compare, (the moon and the earth) there is little doubt but that this planet also has inequalities on its surface, which may be, for what we can say to the contrary, very considerable.

The real diameter of Venus, I should think, may be inferred with great confidence, from the measures I took with the 20-feet reflector, in the morning of the 24th of November, 1791; which, when reduced to the mean distance of the earth, give 18″,79 for the apparent diameter of this planet.

This result is rather remarkable, as it seems to prove that Venus is a little larger than the earth, instead of being a little less, as has been supposed; yet, upon the nicest scrutiny, I cannot find fault with the measures. The planet was put between the two wires of the micrometer, which were outward tangents; and they were, after each measure, shut, so as to meet with the same edge, and in the same place where the planet was measured. In this situation the proper deduction, for not being central measures, was pointed out by the index plate. The transits of the 25th were corrected for a small concavity of the wires, which being pretty thick and stubborn, were not strained sufficiently to make them quite straight, the amount of which was also ascertained by an examination of the division where the wires closed at the ends, and where they closed in the centre. The zero was, with equal precaution, referred to a point at an equal distance from the contact of the wires on each side; for they are at liberty to pass over each other, without occasioning any derangement. The *shake*, or *play*, of the screw is less than 3-tenths of a division.

The two planets, however, are so nearly of an equal size, that it would be necessary to repeat our measures of the diameter of Venus, in the most favourable circumstances, and with micrometers adjusted to the utmost degree of precision, in order to decide with perfect confidence that she is, as appears most likely, larger than the earth.

The remarkable phænomenon of the bright margin of Venus, I find, has not been noticed by the author we have referred to: on the contrary, it is said, page 310, "*this light appears strongest at the outward limb a b c, from whence it decreases gradually, and in a regular progression, towards the interior edge, or terminator.*" But the luminous border, as I have described it, in the observations of the 9th, 16th, 20th, and 22d of April, does not in the least agree with the above representation.

With regard to the cause of this appearance, I believe that I may venture to ascribe it to the atmosphere of Venus, which, like our own, is probably replete with matter that reflects and refracts light copiously in all directions. Therefore on the border, where we have an oblique view of it, there will of consequence be an increase of this luminous appearance. I suppose the bright belts, and polar regions of Jupiter, for instance, which have a greater light than the faint streaks, or yellow belts, on that planet, to be the parts where its atmosphere is most filled with clouds, while the latter are probably those regions which are free from them, and admit the sun to shine on the planet; by which means we have the reflection of the real surface, which I take to be generally less luminous.

If this conjecture be well founded, we see the reason why spots on Venus are so seldom to be perceived. For, this planet having a dense atmosphere, its real surface will com-

Fig. 1.

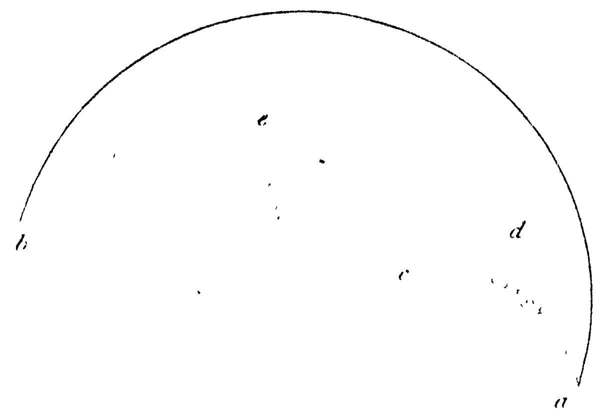
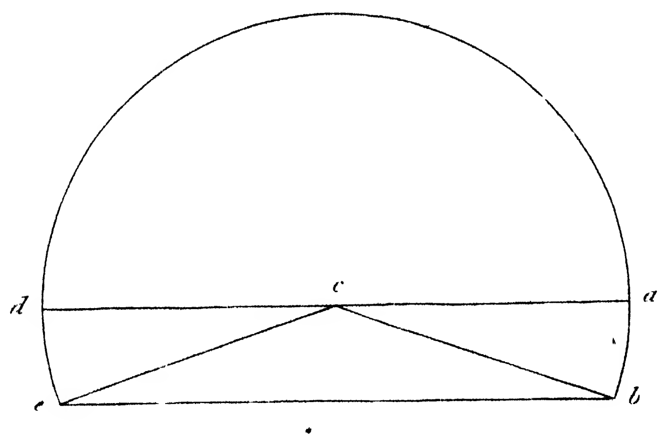


Fig. 2.



monly be enveloped by it, so as not to present us with any variety of appearances. This also points out the reason why the spots, when any such there are, appear generally of a darker colour than the rest of the body.

XIX. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland. *By* Thomas Barker, *Esq.; with the Rain in* Surrey *and* Hampshire, *for the Year* 1792; *and a Comparison of wet Seasons. Communicated by* Thomas White, *Esq. F. R. S.*

Read June 20, 1793.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.				Surrey.	Hampshire.	
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	Lyndon.	South Lambeth.	Selbourn.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.	Inches.	Inches.	Inches.
Jan.	Morn.	29,92	28,47	29,18	47½	30	39	46½	16	34½	2,097	2,51	6,07	4,47
	Aftern.				49	30½	39½	51½	25	36½				
Feb.	Morn.	94	29,04	48	47½	32	41	47½	16½	35	0,712	1, 5	1,68	1, 6
	Aftern.				49	34	42	55	26	42½				
Mar.	Morn.	30,00	28,53	26	50	35	44	48½	25½	39	1,096	2,13	6,70	2,92
	Aftern.				51	35½	45	57	30½	47½				
Apr.	Morn.	29,85	72	42	60	43½	51	56	36½	46	4,042	2, 4	4,08	2, 9
	Aftern.				62	44	53	71	39	57				
May	Morn.	91	77	49	58½	45	50½	58	36½	47½	1,660	1,49	3,00	2,51
	Aftern.				62	46	53	68	45	57				
June	Morn.	88	97	46	63	50	54½	64½	47	53	4,043	1,45	2,78	3,17
	Aftern.				67	53	57	77½	49	62½				
July	Morn.	71	29,13	41	65	53	59½	66½	52	57½	3,674	3,98	5,16	3,81
	Aftern.				68	57½	61	78	57½	67½				
Aug.	Morn.	83	28,89	48	69	57	62½	67½	50	58½	2,861	2,86	4,25	2,52
	Aftern.				73	59½	65	79½	61	70				
Sep.	Morn.	85	57	30	61½	48½	55	60	41½	50	3,977	2,66	5,53	3,93
	Aftern.				63½	50	56	68½	48	58				
Oct.	Morn.	97	72	34	58	46	49	57	35	45½	1,756		5,55	4, 6
	Aftern.				59	46	50½	66	46	52				
Nov.	Morn.	91	78	52	51½	40½	46	50½	31½	42½	0,761		1,65	90
	Aftern.				53	39½	46½	56	37½	47				
Dec.	Morn.	85	50	31	48	36	41	52	29	39	2,723		2,11	1,40
	Aftern.				48½	36	42	54	31	41½				
											29,402		48,56	32,84

THE winter was a severe one; there was a sharp frost every month from December to March, chiefly between the full and the new moons, and the intervals were often stormy and wet; but those in February, both at the middle and latter end of the month, were milder, and less wet. The beginning of March continued mild, with frequent though small rains; then followed as sharp a frost, for a week, as any in the winter. After that stormy weather into April, but warm and growing; till a violent thunder storm toward Stamford the 13th, and two days continued rain here, and in most other places, about the 18th, renewed the wet season; which lasted all summer, and was perhaps wetter in many places than here, for we had no heavy thunder storms all the summer, as they had in some parts. Whenever there was thunder this year, it was almost always cold after it, and often cold weather without it; very little sunshine, and many sharp frosty mornings both in May and June, which cut off the apples after they appeared to be set. The greatest rains this summer were after the middle of April; before the middle of May; about the 8th of June; the 21st of July; the 18th of August; and 14th of September: those in April, June, and July, made floods, the two latter of which did great damage to the meadow hay; and there were frequent, sometimes almost daily, lesser rains. The intervals of fair and fine weather were short, and not many, and those not always warm; the beginning of May, and about the 21st; the beginning and end of June; the beginning of July; and, what was the finest time this summer, the first half of August. During this, in general so very wet a season, the hay and harvest were got in, and, where they were not flooded, I think with less damage than might have been expected. The latter hay was got up during the fine time in August; some of the harvest in a tolerable time the beginning of September; and what was delayed by the almost daily rains for two-thirds of September, was finished in a fine time the beginning of October; the crop of wheat was tolerable well, but barley, oats, and peas, were dear.

This year was the wettest since 1782, which, with 1774, and some others, exceeded it; and this, like those two years, began to grow less wet the beginning of October. Yet the frequent rains after that, though less in quantity, kept the ground from drying, which was already too wet, and the roads continued uncommonly torn up all winter; and December, being wetter, increased it. The last six weeks of the year were in general dark and cloudy, or misty; very little sun, and not much frost, and so far seems to promise an open winter; but December was a stormy time; several great ones, and some great rains and floods.

A Comparison of wet Seasons.

Twelve Months.

No. I.

	1774.		1782.		1792.	
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
January	3,308		2,333		2,097	
February	1,946	5,254	0,636	2,969	0,712	2,809
March	2,728	7,982	1,923	4,892	1,096	3,905
April	1,523	9,505	6,125	11,017	4,042	7,947
May	3,142	12,647	5,722	16,739	1,660	9,607
June	2,483	15,130	1,295	18,034	4,043	13,650
July	3,227	18,357	2,697	20,731	3,674	17,324
August	3,910	22,267	3,114	23,845	2,861	20,185
Septem.	8,000	30,267	5,151	28,996	3,977	24,162
October	1,156	31,423	1,502	30,498	1,756	25,918
Novem.	1,530	32,953	1,074	31,572	0,761	26,679
Decem.	2,282	35,235	0,517	32,089	2,723	29,402
	1773	29,376				
	1775	31,699				
Three years		96,310				

No. II.

Oct. 3, 1773 to Oct. 2, 1774.		Dec. 1774 to Nov. 1775.		Oct. 1791 to Sept. 1792.		Feb. 1763 to Jan. 1764.		1768.	
	Inches.		Inches.		Inches.		Inches.		Inches.
Oct. 3	2,615	Dec.	2,282	Oct.	3,319	Feb.	2,882	Jan.	2,834
Nov.	3,605	Jan.	1,973	Nov.	4,231	Mar.	0,919	Feb.	3,062
Dec.	2,897	Feb.	2,522	Dec.	1,150	April	0,692	March	0,391
Jan.	3,308	Mar.	1,728	Jan.	2,097	May	2,304	April	2,023
Feb.	1,946	April	1,035	Feb.	0,712	June	2,426	May	1,622
March	2,728	May	0,900	March	1,096	July	5,657	June	4,521
April	1,523	June	0,887	April	4,042	Aug.	2,929	July	2,402
May	3,142	July	4,078	May	1,660	Sept.	3,307	Aug.	1,720
June	2,483	Aug.	4,760	June	4,043	Oct.	1,606	Sept.	3,025
July	3,227	Sep.	5,670	July	3,674	Nov.	1,894	Oct.	3,119
August	3,910	Oct.	3,480	Aug.	2,861	Dec.	3,525	Nov.	4,040
Septem.	8,000	Nov.	3,570	Sept.	3,977	Jan.	3,984	Dec.	2,146
Oct. 1 & 2	0,340								
			32,885		32,862		32,125		30,905
	39,724								

No. III.

Three years.

	May 9, 1773, to May 8, 1776.				17 months.—May 9, 1773, to Oct. 8, 1774.	
	1773.	1774.	1775.	1776.	1773.	1774.
	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
Jan.		3,308	1,973	2,511		3,308
Feb.		1,946	2,522	3,195		1,946
March		2,728	1,728	1,518		2,728
April		1,523	1,035	0,887		1,523
May	6,770	3,142	0,900	0,860	6,770	3,142
June	2,389	2,483	0,887		2,389	2,483
July	1,077	3,227	4,078		1,077	3,227
August	3,379	3,910	4,760		3,379	3,910
Septem.	2,812	8,000	5,670		2,812	8,000
October	2,621	1,156	3,480		2,621	0,460
Novem.	3,605	1,530	3,570		3,605	
Decem.	2,897	2,282	1,096		2,897	
	25,550	35,235	31,699	8,971	25,550	30,727
	Three years time 101,455					56,277

No. IV.

Nine months.

Jan. 6, to Oc. 6, 1774.		Jul. 1775 to Mar. 1776.		Jan. 1782 to Sept.		May, 1773 to Jan. 1774.		May, 1763 to Jan. 1764.		April, 1768 to Dec.		Jan. 1792 to Sept.	
	Inches.		Inches.		Inches.		Inches.		Inches.		Inches.		Inches.
Jan.	3,308	Jul.	4,078	Jan.	2,333	May	6,843	May	2,304	Apr.	2,023	Jan.	2,097
Feb.	1,946	Aug	4,760	Feb.	0,636	June	2,389	June	2,426	May	1,622	Feb.	0,712
Mar.	2,728	Sep.	5,670	Mar.	1,923	July	1,077	July	5,657	June	4,521	Mar.	1,096
Apr.	1,523	Oct.	3,480	Apr.	6,125	Aug	3,379	Aug	2,929	July	2,402	Apr.	4,042
May	3,142	Nov.	3,570	May	5,722	Sep.	2,812	Sep.	3,307	Aug	1,720	May	1,660
June	2,483	Dec.	1,096	June	1,295	Oct.	2,621	Oct.	1,606	Sep.	3,025	June	4,043
July	3,227	Jan.	2,511	July	2,697	Nov.	3,605	Nov.	1,894	Oct.	3,119	July	3,674
Au.	3,910	Feb.	3,195	Aug	3,114	Dec.	2,897	Dec.	3,525	Nov.	4,040	Aug	2,861
Sep.	8,000	Mar.	1,518	Sep.	5,151	Jan.	3,308	Jan.	3,984	Dec.	2,146	Sep.	3,977
Oct.	0,460		29,878		28,996		28,931		27,632		24,618		24,162
	30,727												

No. V.

Six months.

April 12, 1782, to Oct. 11.		July to Dec. 1775.		April to Sept. 1774.		April to Sept. 1792.	
	Inches.		Inches.		Inches.		Inches.
April 12	5,375	July	4,078	April	1,523	April	4,042
May	5,722	Aug.	4,760	May	3,142	May	1,660
June	1,295	Sept.	5,670	June	2,483	June	4,043
July	2,697	Oct.	3,480	July	3,227	July	3,674
August	3,114	Nov.	3,570	Aug.	3,910	Aug.	2,861
Septem.	5,151	Dec.	1,096	Sept.	8,000	Sept.	3,977
Oct. to 11	0,950		22,654		22,285		20,257
	24,304						

No. VI.

Three months.

July 3, 1774, to Oct. 2.		1775.		1782.		1770.	
	Inches.		Inches.		Inches.		Inches.
July 3	3,227	July	4,078	Mar.	1,923	Oct.	3,114
August	3,910	Aug.	4,760	Apr.	6,125	Nov.	7,818
Septemb.	8,000	Sept.	5,670	May	5,722	Dec.	2,613
Oct. 1 & 2	0,340		14,508		13,770		13,545
	15,477						

1763.		1737.		1784.		May 25, 1792, to Aug. 24.	
	Inches.		Inches.		Inches.		Inches.
July	5,657	Aug.	6,300	May	2,890	May 25	0,770
Aug.	2,929	Sept.	3,465	June	3,810	June	4,043
Sept.	3,307	Oct.	2,025	July	5,080	July	3,674
	11,893		11,790		11,780	Au. to 24	2,511
							10,998

No. VII.

One month.

	Inches.
1774 September 3	7,930
to Oct. 2	0,340
	8,270
1770 November 6	7,818
to Dec. 5	0,410
	8,228
1773 May - -	6,843
1736 July -	6,550
1737 August -	6,300
1782 April -	6,125
1757 August -	6,057
1782 May -	5,722
1775 September	5,670
1763 July - -	5,657
1743 July - -	5,230
1776 August -	5,200
1782 September	5,151
1792 August 16	2,762
to Sept. 15	2,346
	5,108

No. VIII.

Abstract.

	Inches.
Three years time - -	101,455
Three calendar years, 1773, 1774, and 1775 - -	96,310
Seventeen months - -	56,277
Twelve months - -	39,724
Nine months - -	30,727
Six months - -	24,304
Three months - - -	15,477
One month - - -	8,270

The year 1792 was a very wet one, and by many imagined to exceed all others, but that does not appear to be fact; the wet of last year is fresh in memory, that of former years is more forgotten. It might seem the wetter, because the autumn of 1791 was wet, so that there was a long continuance of it; and perhaps there might be more rain in some other places than here, as we had no great thunder storms all the summer at this place, which they had in several parts, some not many miles off. The wettest years here were about 1774 and 1782, which I have therefore compared with last year, in No. I. where I have set down the whole rain, and cast up the sum, from January the first, to the end of every month, in each year: and it appears, that to the end of January, to the end of February, and of March, the wettest was 1774, the next 1782, and 1792 was less wet than either of them. The very wet April and May in 1782 altered the order of them; and to the end of April, of May, of June, of July, and of August, the wettest was 1782; the next 1774; and the last 1792. September, 1774, that wettest of all months in fifty-seven years, altered the order again to 1774, 1782, 1792; and it continued so to the end of the year. In No. II. I have given some of the greatest twelve months, whether beginning with January or not; and the greatest 365 days is from October 3, 1773 to October 2, 1774, which is 39,724 inches; and all that I have here given exceed 1792. In No. III. is the greatest three years, from May 9, 1773 to May 8, 1776, which is 101,455 inches; and the greatest seventeen months, from May 9, 1773 to October 8, 1774, is 56,277 inches. In No. IV. are the greatest nine months, January 6 to October 6, 1774, 30,727 inches; and several others, to 1792, 24,162 inches. In No. V. are several of the greatest six months, from 1774, 24,304 inches, to 1792, 20,257 inches. In No. VI. are several of the greatest three months, from 15,477 inches in 1774, to 10,998 inches in 1792. The greatest month last year was, from August 16 to September 15, 5,108 inches, but I have had thirteen greater; the most of all was in 1774, 8,270 inches; the rest are set down in order in No. VII.; and the last is that in 1792. Lastly, in No. VIII. I have set down together the wettest times in all the several cases.

At Selbourn, between Alton and Petersfield, in Hampshire, which lies at the NE foot of a steep hill, that rises an hundred yards perpendicular above it, they have half as much more rain as I have; there was $48\frac{1}{2}$ inches last year, as it is set down in the first page; but they had $50\frac{1}{4}$ inches in 1782, which is something more. But I was surprised to see, in the Supplement to the Gentleman's Magazine, page 1197, that Mr. Gough says there was $83\frac{1}{2}$ inches of rain at Kendal last year. This is an astonishing quantity; though it is a hilly country, it is almost four times my common year, and above double the greatest; and I should have thought it enough, in latitude 54°, to have made the whole country a marsh.

PRESENTS

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1792.	PRESENTS.	DONORS.
Nov. 8.	Memoires de l'Académie Royale des Sciences. Août, 1786, jusqu'à la fin de 1787. Berlin, 1792. 4°	The Royal Academy of Sciences of Berlin.
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	Discours qui ont été lus dans l'Assemblée publique de l'Académie des Sciences de Berlin, tenue le 26 Janv. 1792. Berlin, 1792. 4°	Comte de Hertzberg, F. R. S.
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	Medical Facts and Observations, Vol. III. London, 1792. 8°	Samuel Foart Simmons, M. D. F. R. S.
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	The Sportsman's Directory, or Tractate on Gunpowder, by G. Montagu. London, 1792. 8°	George Montagu, Esq.
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Nov. 15.	Discours qui a remporté le Prix à l'Academie de Chalons en l'Année 1783. Beauvais, 1789. 4°	M. Bucquet, of Beauvais.
	A Descriptive Account of a Descent made into Penpark-hole, in the year 1775. Bristol, 1792. 8°	Mr. George Symes Catcott.
22.	A Botanical Arrangement of British Plants, by W. Withering, Vol. III. Birmingham, 1792. 8°	William Withering, M. D. F. R. S.
	Commentarii de Rebus in Scientia Naturali et Medicina gestis, Volumen XXXIII. Lipsiæ, 1791. 8°	Mr. Hurlock, F. R. S.
	Museum Leverianum, No. 3 and 4. 4°	Mr. James Parkinson.
	J. Dickson Fasciculus secundus Plantarum Cryptogamicarum Britanniæ. Londini, 1790. 4°	Mr. James Dickson.
Dec. 6.	Cases of the Hydrocele, by T. Keate. London, 1788. 8°	T. Keate, Esq.
	Principles of Surgery, by J. Pearson, Part I. London, 1788. 8°	Mr. John Pearson.
	Dissertation sur les Causes qui ont produit l'Espece de Contradiction que l'on remarque entre deux Decrets de l'Assemblée Nationale. 8°	M. Hourcastremé.
13.	Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge. London, 1793. 8°	The Society for the Improvement of Medical and Chirurgical Knowledge.
	Collections for an History of Sandwich, by W. Boys. Canterbury, 1792. 4°	William Boys, Esq.
	Memoir on the Use of the Thermometer in Navigation, by J. Williams. Philadelphia, 1792. 4°	Mr. Williams, Secretary to the American Philosophical Society.
20.	Astronomisches Jahrbuch für das Jahr 1795, von J. E. Bode. Berlin, 1792. 8°	Mr. J. E. Bode, F. R. S.
	Projet des Orbites Paraboliques de 72 Cometes, dont le Cours a été calculé jusqu'ici, reduite au Plan de l'Orbite de la Terre, par M. Bode. fol. pat.	————————
	A Treatise on the Hydrocele, by J. Earle. London, 1791. 8°	James Earle, Esq.
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1793. *Jan.* 10.	Kongl. Vetenskaps Academiens Nya Handlingar, Tom. XII. for 1791, 3d and 4th quarter;	The Royal Academy of Sciences of Stockholm.

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	Medical Commentaries for the Year 1792, collected by A. Duncan. Edinburgh, 1793. 8°	Andrew Duncan, M. D.
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	A Sermon preached before the Lords, January 30, 1793, by Samuel Lord Bishop of St. David's. London, 1793. 4°	The Lord Bishop of St. David's, F. R. S.
	An Essay on Generation, by J. F. Blumenbach, translated from the German. London, 1793 12°	A. Crichton, M. D.
	Museum Leverianum, No. 5. 4°	Mr. James Parkinson.
March 7.	Elements of Jurisprudence. London, 1783. 4°	Richard Wooddeson, D. C. L.
	A Systematical View of the Laws of England, by R. Wooddeson, III. Vols. London, 1792. 8°	———————
	De Calculo Integralium, Exercitatio P. Ferronii. Florentiæ, 1792. 4°	Professor Ferroni, of Pisa.
	Histoire de la Noblesse Héréditaire des Gaulois, par C. J. de Bevy. Tome I. Londres, 1791. 4°	Abbé de Bevy.
14.	Elogio d'Amerigo Vespucci, del P. Stan. Canovai, 1790. 4°	Father Gaetano del Ricca, of Siena.
April 11.	The literary Life of the late Thomas Pennant, Esq. by himself. London, 1793. 4°	Thomas Pennant, Esq. F. R. S.
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	E. F. von Hertzberg; auszug aus Weidlichs biographie der jeztlebenden Rechtsgelehrten. 8°	————
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	Lectures on the Elements of Farriery, by C. Vial de Sainbel. London, 1793. 4°; with a set of Models of Horses Hoofs and Shoes.	Mr. Vial de Sainbel.
18.	Historical View of Plans for the Government of British India, 1792. 4°	The Right Hon. Henry Dundas.
May 9.	Substance of the Speech of the Right Honourable Henry Dundas, on the British Government and Trade in the East Indies, April 23. 1793. 4°	John Bruce, Esq. F. R. S.
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	Practical Essays on the Management of Pregnancy and Labour, by J. Clarke. London, 1793. 8°	John Clarke, M. D.
16.	The Military Antiquities of the Romans in North Britain, by W. Roy. London, 1793. fol.	The Society of Antiquaries.
	The Theory and Practice of finding the Longitude at Sea or Land, by A. Mackay. London, 1793. 8°	Mr. Andrew Mackay.
30.	The Transactions of the Royal Irish Academy, Vol. IV. Dublin. 4°	The Royal Irish Academy.
	Considerations on the Utility of the National Debt, by E. King. London, 1793. 8°	Edward King, Esq. F. R. S.
June 6.	The antecedental Calculus, by J. Glenie. London, 1793. 4°	James Glenie, Esq. F. R. S.
13.	Analyse du Systême Absorbant ou Lymphatique, par R. Desgenettes. Paris, 1792. 12°	René Desgenettes, M. D.
20.	Oriental Repertory, Vol. I. published by A. Dalrymple. London, 1793. 4°	Alexander Dalrymple, Esq. F. R. S.
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	On Electric Atmospheres, by E. Peart. Gainsborough, 1793. 8°	E. Peart, M. D.
	Theorie des Vents, par M. le Chev. de la Coudraye. Fontenay, 1786. 8°	Chevalier de la Coudraye.

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From the Press of
W. BULMER & Co.
Cleveland-Row, St. James's.

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